SCIENCE.

FRIDAY, APRIL 18, 1884.

COMMENT AND CRITICISM.

TRAVELLERS in the west during the past few years will surely have met the statement that the rainfall of the dry region beyond the Mississippi is increasing. Many western settlers express the hasty conclusion that the change is a steadily progressing one, and is due to the cultivation of the ground; and the more venturesome theorists explain the increase as an effect of the better equalization of electric conditions of the atmosphere, as allowed by the laying of iron rails and the stretching of iron wires across the plains. The natural extension of these theories pictures the plains in the near future redeemed from their present unprofitable dryness by persistent occupation. It is well to set these unwarrantable fancies face to face with the matter-of-fact statistics lately published by the signal-service; for, whatever the doubtful possibilities of man's power may be, the connection of such small artificial changes with variation of rainfall in the relation of cause and effect is in the last degree questionable. There is not the least reason to think that the *régime* of the winds and rain can be permanently affected so easily, or that any progressive change is going on so rapidly as to make itself felt in a decade of years.

As to the fact of variation in rainfall from year to year, there is, of course, no question: this is a most ordinary condition, especially in regions of moderate precipitation, where a good share of the annual fall may be gathered from a single storm. But, beyond this, the tables lately published show certain wide-spread variations of importance. Signal-service note no. vii., prepared by Mr. H. A. Hazen, discusses the variation of rainfall west of the Mississippi River, as shown in the records of nearly seventy stations from 1871 to 1882 inclusive, with the conclusion that "many more years of observation will be needed, as well as many additional stations, . . . before any secular variation can be fully established." The tables show irregularities in the amount of rain roughly conformable over large areas. From 1871 to 1873 there was a general deficiency; about 1875, 1876, 1877, there was corresponding excess; 1878 or 1879 was notably dry again; and from then to 1882 there was a general increase, but not above the previous maximum. It is therefore now altogether premature to regard the variation of rainfall as any thing but what may be, in the present condition of meteorology, properly termed 'accidental;' that is, due to subordinate causes not yet determined, and not to any progressively increasing factor, like cultivation or rail-laying.

"THIS brings them dangerously near the American category of 'dead heads;' but, lest they should incur the additional reproach of being 'free lunchers,' they will be allowed to pay a sum not exceeding two shillings for a 'square meal.'" This, from the comments of the London *Times* on the great American or Canadian promised hospitality to the British association, on its approaching visit to Montreal, has reference to the alluring pictures held up of the trip west on the Canadian Pacific railroad. 'Allowed to pay two shillings' — what a favor! — for a meal in a crowded saloon of a crowded steamer, first table, second table, third table, as the promptness and skill of the traveller in 'jumping' a chair may justify. 'For a square meal' in a tent, two hundred miles from the nearest cow, how gladly the easy-going English traveller will pay his two shillings, — money which would bring a cup, a saucer, a plate, a knife, a fork, in England, but in Canada's backwoods restaurant may only secure a saucer. How happy our English cousin with a saucer, and a blushing rustic before him to inform him that there is bread,

perhaps, to fill it, but no beer, no milk, no good water, only bad tea and bad coffee! A few days of such living, a glance into a 'muskeag,' a ride over the streets of Winnipeg, and how mistaken will appear the managers of the association in planning an American meeting!

THE visits of foreign astronomers to observatories on American soil have of late years been very frequent; and it is not, perhaps, too much to say that the impressions they have carried away have in the main been of a pleasantly favorable and in some instances of even a surprising character. Occasionally they have made free to express themselves with regard to the somewhat rapid development of, and the future outlook for, their science in this country; but only infrequently have their opinions and criticisms been placed on permanent record. During the latter part of the summer of 1883, Dr. Ralph Copeland, astronomer to the Earl of Crawford and Balcarres, and editor of the lately discontinued journal *Copernicus*, passed through the United States, and visited a goodly number of the more active observatories, among them those at Cambridge, Washington, Princeton, Albany, and Clinton. His general impressions, as he modestly styles them, are far from uninteresting; and, while there is much that has been suggested before, American astronomy has not yet advanced to a stage where no opportunity offers for advantage from such suggestions.

THE decision of our treasury department, by which fine weights, such as are necessarily used by every chemist, are for customs purposes not to be regarded as 'philosophical apparatus,' but as articles worked in metal, is as plain a violation of the spirit of the law as could well be imagined. A similar case presented itself a few years ago, when a college imported bottles for use in the chemical laboratory. There was no doubt about the fact that the bottles were to be used for purely scientific purposes. They were without question 'philosophical apparatus' in the sense in which that expression is used in the tariff law; and yet the secretary decided that the bottles were to be classed as bottles, and not as 'philosophical apparatus;' and the college had to pay duty on them to the extent of forty per cent *ad valorem*. If the law, as it stands, has any object, that object is, by relieving educational institutions from certain burdens, to encourage the spread of knowledge. Without this object, the law is meaningless. By what right, then, does the secretary of the treasury decide that educational institutions shall not have the benefit of the law?

THE child, seeing for the first time the evening star, exclaims, 'O mamma! God has made a star.' How should this wondering admiration of the novelties to the opening mind be received? The parent has seen many a star, has possibly a great objection to stars from being obliged to watch them for hours. Next morning the child may rush in with open eyes, and demand the mother's sympathy, in its excitement over a passing wagon heavily loaded, and drawn by six horses; or at the quaint humanism of the organ-grinder's companion at the street-corner. These, again, are familiar experiences to the mother, and of themselves would only call forth a moan at the rumbling of the wheels or at the squeaking of the pipes. The child feels hurt if sent away with only a 'Yes, dear,' or 'Run along,' and next time wonders to itself, and another time not at all. To the teacher and to the editor rushes the boy of all ages, and with trembling voice announces that 'the thickness of a mercury-drop on a glass plate is constant,' and suggests its adoption as a standard of length, or that a rotating wheel resists a change in the plane of its rotation, and immediately builds upon his experiment a perpetual motion, or that he has found some relation between the physical constants of a few bodies, and warps the others to fit some preconceived theory. How is all this enthusiasm to be met? With the child, it is the evidence of an active intellect, and gives promise for the future, and may be enjoyed with it; with the boy of tender age, there is no harm in pointing out that he has

come on the stage at a comparatively late period in the world's activity, and that it would be well to inquire, before bounding with joy at his new possession, whether it may not be an old one in the world's stock of knowledge, or even valueless; but for the old boy, the incorrigible old boy, who is constantly popping up with his theory of comets, his theory of the gyroscope, or his very important measurements of the thickness of a mercury-drop, what can be done? His questions and talk may show evidences of an active mind, but of a mind working within a Chinese wall of self-sufficiency. He feels intensely indignant when told to examine the records, and compare his work with that of others. He is only working as every philosopher formerly worked, within himself; but at this age he is — a bore.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

The use of the method of rates in mathematical teaching.

IN *Science* for March 28, Professor Wood, referring to the method of rates, says, "There is the same difficulty in the fundamental conception as in the infinitesimal method;" and he represents a student as asking the questions, "'In a mathematically perfect engine, does the piston stop at the end of the stroke?' 'Does it remain at rest at any time?' 'How can it reverse its motion, if it does not stop?' 'How can it cease going in one direction, and move in the opposite direction, without stopping between the two motions?'" This difficulty, if it exists, must be met in the teaching of mechanics, and may therefore be discussed apart from the question whether it be advisable to found the differential calculus upon the conception of velocity. The form of the questions which Professor Wood puts into the mouth of the student somewhat puzzles me. I can but suppose that Professor Wood answers 'Yes' to the first question; but, in that case, how can the student ask the third or fourth question? The difficulty must lie in the answer to the second question, 'Does it remain at rest at any time?' It would not be safe to answer this question at all in this form, because it indicates a confusion of mind in the use of the word 'time.' 'At any time' might mean 'at any instant;' but the use of the word 'remain' shows that the student probably meant 'remain at rest *for* any time;' that is, for any interval of time. To the question thus amended, we can safely answer, 'No.' But having already admitted that the piston does stop at a certain instant, namely, 'the end of the stroke,' the student has no occasion to ask the third or fourth question. Of course, a student may be easily puzzled by the metaphysical subtleties and sophistries by which a certain school of philosophy persuaded itself that motion was impossible; but, left to himself, he has no more difficulty in appreciating the difference between an 'instant' and an 'interval' of time than he has in distinguishing between a point and a line in geometry.

Farther on in his letter, Professor Wood asks, 'Does change in the rate of motion take place *at* an instant, or *during* an instant?' It seems to me that if he will dispense with the colloquial use of the word 'instant' for a small interval of time, and substitute 'during an interval,' the so-called difficulty will disappear. Do his students ever ask whether the positive and negative parts of the axis of x are separated by a point, or by a space?

WM. WOOLSEY JOHNSON.

Annapolis, April 5.

Paleozoic high tides.

Your reviewer of the *Geographisches jahrbuch*, referred to by Professor Newberry in *Science* (No. 61, p. 402), was led, by the evidence given in brief below, to the conclusion that tides higher than those now observed, produced in the way explained by G. H. Darwin and illustrated by Ball, had occurred within paleozoic time. It was not, however, his intention to accept the gigantic tides described by Ball, but simply tides *significantly* stronger than those of the present time; for these seem not only warranted, but required, by the spread of paleozoic strata.

Soundings and dredgings, as summarized, for example, in the *Lithologie du fond des mers*, by Delesse, prove that the coarser land-derived sediments, such as form conglomerates and sandstones, are deposited within a moderate distance of their origin, excepting in narrow tide-ways, such as the English Channel, where they stretch out farther; elsewhere, pebbles especially fall within a very narrow fringe along shore. The paleozoic strata of the eastern United States give ample evidence of submarine transportation of land-derived sediments certainly three hundred miles from their source, of sands at least half this distance, and of clean sands with pebbles certainly a hundred miles; and this when measuring only from the present south-western margin of the Cambrian strata. In this regard, the Medina, Oriskany, and carboniferous sandstones and conglomerates, which overlie calcareous or shaly strata, from which their siliceous elements could not have been derived, give very much stronger evidence than that obtained from the Potsdam sandstone, which was formed during the advance of the sea over an old land-surface, whose local waste may have formed this basal deposit close along shore. I must consequently persist in believing that the spread of pebbles and sand over the old sea-floor during the above-named epochs implies a greater transporting-force than is now known in the modern oceans.

The Jurassic sandstones of the Colorado plateau were, according to Capt. Dutton, deposited with very little shaly admixture over an area of thirty-five thousand square miles. A liberal estimate of the Bay of Fundy gives it under four thousand square miles, and its deposits are rather muddy than sandy; that is, muds such as were washed out of the old Jurassic basin are allowed to accumulate in the Bay of Fundy. Whether the tides were much stronger in Jurassic time than now, is perhaps an open question; but that marine transportation was then stronger seems, at least from this example, very probable.

In looking for a cause of former greater activity in the ocean, we find it only in the possible variation of the tides and currents, unless recourse be had to the older cataclysmic theories. Increase in the velocity of currents needs stronger differences between polar and equatorial temperatures, or an advantageous con-

figuration of shores. Our paleozoic ocean was too broad to hurry its currents by crowding them. There is no probability that differences of *ocean* temperature in the past have been great enough seriously to increase the currents; and the little that is known of past aerial temperatures is not enough to insure steeper barometric gradients for stronger winds. As to the velocity of the winds being proportional to the rotation or size of their planet, I must venture to differ from Mr. Darwin (*Nature*, xxv. 1882, 213): for barometric gradients would be steeper on a small planet than on a large one; and the deflecting force, coming from the planet's rotation, depends, not on its size, but on its angular velocity. Moreover, this force does not significantly affect the wind's velocity, but only its direction; and if the earth turned faster, as it may have formerly, the course of the trade winds would be *flattened* (made more oblique to the meridians), but their velocity would not be materially changed, as has been shown by Ferrel. It does not, therefore, seem safe to count on stronger ocean-currents in the past, until it can be shown that the difference between polar and equatorial temperatures was formerly greater than it now is.

But with tides the case is different. There has been found a mechanism by which the tides have decreased automatically from a former greater strength, and I feel that such a contribution to former greater activity in the ocean is to be welcomed in physical geology. It is not a question of six hundred foot tides, by whose devastating strength Mr. Ball has weakened his argument, but of paleozoic marine transportation along the open shores of the ocean, of greater force than is now found; and to this end the old tides promise effective aid. W. M. DAVIS.

Cambridge, April 8.

Transmission of long or inaudible sound-waves.

A simple method of testing whether the atmospheric wave (which, it is claimed, passed around the earth in less than thirty-six hours) had its origin at, and was due to an explosion of, the volcano Krakatoa, would be to examine the previous records of the self-recording instruments for those particular times at which the waves caused by the explosions of some of the larger powder-mines would reach a given locality.

That explosions of this kind cause disturbances which are made manifest (without the aid of any delicate instruments) at localities many miles from the place of disaster is a well-known fact. S.

Tornado in western North Carolina.

On Tuesday, March 25, about five P.M., a tornado passed through portions of Catawba and Iredell counties, extending in a due east course for twenty-five miles.

The first evidence of a destructive storm is two miles and three-fourths west of the town of Newton, the highest point of land east of Baker's Ridge, which is twelve miles to the west. The fallen trees showed two distinct currents of wind, — the one from a few degrees north of west, the other south-west. No evidence of a rotary motion was observed until within three-fourths of a mile of Newton, which, however, was only in a limited area. In the town, and east of it, the rotary motion was decided and destructive.

A very extended and severe hail-storm extended all along the track of the tornado on the north or left side, slowly moving south, reaching the path of the storm. The day had been unusually warm; wind south, shifting to south-west. Several persons witnessed the meeting of the rapidly moving clouds from the south-west with the hail-cloud; also the formation of the descending tornado-cloud. Before it reached the earth, portions became detached, and descended to the earth, afterwards united, and moved forward unbroken. While passing through Newton, the form of the cloud was that of an hourglass, the lower end considerably retarded, the middle portion waving. Immediately east of the town there is a valley; and, when the cloud passed over it, it became erect and funnel-shaped. The surface of the country over which the storm passed is quite diversified. Valleys nearly in the direction of the storm's path were able to deflect its course slightly. The highest points showed evidence of greatest force, though frequently the trees were felled in the lowest parts of the valleys.

The after-wind was but slight. Several houses were lifted from the lower floor and carried away, leaving the occupants unhurt, and not blown along by an after-wind.

The left side of the track is quite sharply defined, while the right extends to a much greater distance, and gradually all trace disappears. The width of the path is from five hundred yards to a mile, though the more destructive part is from a hundred and fifty to five hundred yards.

The damage to houses, barns, timber, and fencing, was very great; nothing being able to withstand the force of the storm except the small trees.

J. W. GORE.

University of North Carolina,
April 8.

Osteology of the cormorant.

If Dr. Gill had read the literature on the cormorant before writing to *Science*, he would have learned that I was following Selenka, and that my reference was all-sufficient for the purpose; namely, a reference to a previous figure. Dr. Gill might as easily have referred the committee to the other references found in Carus and Engelmann's *Bibliotheca zoologica*. Those interested in the subject will find my last remarks on the point in dispute in the *Auk* for April.

J. AMORY JEFFRIES.

The remarks of Dr. Gill, which are contained in his letter to *Science*, No. 61, have just been read by me. As one of the persons designated by your correspondent, permit me to thank him for the information he has so timely tendered.

A certain amount of reprehension always attaches to a laborer in any field of science if he is found not to be thoroughly acquainted with the literature of his subject. This censure is well deserved, particularly if no good excuse exists for such ignorance. The language used by Dr. Gill in his letter seems to bear with it this charge; and, in simple justice to myself, I feel that a few words are demanded from me in answer to it. In my first paper upon the 'Osteology of the cormorant' (ii. 640), I distinctly said that the occipital style is alluded to by Professor Owen, in his 'Anatomy of vertebrates.' That was equivalent to stating the fact that it was universally known to anatomists. The libraries were not available at the time that that article was penned, and I candidly stated in it my ignorance of any figures of the bone in question.

At the time my second notice of this bone was written, the views of other scientific men and the libraries were available; and in a few lines I simply refuted Mr. Jeffries' notion that it was an ossified tendon (ii. 822). Nothing further than this was called

for. In my third and last notice (iii. 143) the manner in which the muscles attached to the occipital style are inserted was alluded to, and it was compared with an ossified ligamentum nuchal. All of this I still maintain. At that time, for lack of material, I had not especially looked into its physiology; and my discussion with Mr. Jeffries closed (Feb. 8, 1884). Since, both through my reading and observation, much has come to my notice of interest with regard to it. Garrod's dissections of Plotus anhinga are very suggestive. Dr. Gill had kindly called my attention to Yarrell's paper, before his notice in *Science* appeared, which he had unexpectedly come across while searching for facts to illustrate another subject. Finally, in one of the most useful and reliable of books, Coues' 'Ornithological bibliography,' I had noticed Rudolphi's article; but other matters were engaging my attention then, and reference was not made to it. There are still others. I have already cited Eyton's figure (iii. 143), and believe, at the time Dr. Gill's review of my work appeared, I was hardly entitled to the charge he brings against me in it. I am more and more convinced, every day of my life, that *good illustrations* of such common facts in anatomy are most urgently demanded. R. W. SHUFELDT.

A singular optical phenomenon.

I think it would well repay almost any one to study the beautiful phenomenon so clearly described by 'F. J. S.' (*Science*, No. 57, p. 275), and so suggestively discussed by Professor LeConte (No. 61, p. 404). My own theory of it involves no inverting action, as in the camera, and no *primary* dependence upon binocular vision, but, rather, it resembles the theory of watered silks, or of chords and beats in music. It seems to me geometrically demonstrable; and it includes the phantom meshes' gigantic size, their bewildering motions, their conspicuousness even to eyes out of focus for the actual wires, and the non-appearance in them of objects attached to those wires.

Before the observer are two parallel screens of square-meshed wire netting. The coarser one is seen through the finer, and *the two are at distances from him nearly proportional to the diameters of their meshes, measured from centre to centre of the wires.* To fix the ideas, suppose that he looks with only one eye, seeing the nearer wires black and the farther ones bright: then, if the above proportionality be exact, all the bright wires can be simultaneously eclipsed, each by a separate dark wire; or, upon moving the eye very slightly to the right and upward, all the bright wires will flash into view at once. Now let the observer advance or retire a few inches from this first position, so that the dark wires may subtend visual angles a little larger or smaller than do the corresponding bright ones: several successive bright wires will thus be in view, then one or more will be eclipsed, then several others will be seen, and so on; that is, the phantom screen will be formed, with its great square meshes and shadowy bars.

Next let the observer move slightly to the right: the phantom also moves, but more, and to the right or the left, according as he is in front of or behind his first position. Indeed, the motions of the phantom bars, and the visual angles they subtend, are as if the observer viewed a virtual image whose plane passed through his first position, but imagined it to be some feet in front of him. The size of the virtual image would be very nearly such, that, in it and the farther screen together, there would be as many bars to the foot as in the nearer screen. Its colors would appear to be those of the farther screen, but weaker and oppositely arranged. It would *not* be upside down. Indeed, if 'F. J. S.' will paint the upper wires of the farther screen vermilion, or will hang behind them a blue curtain, then I think that the upper meshes, but not the bars, of the phantom, will be reddened; or the upper bars, and more slightly the meshes, of the phantom, will be bluish. Or, if he will paint the vertical wires red and the horizontal wires yellow, probably the phantom meshes will incline to orange, the vertical phantom bars to yellow, and the horizontal ones to red.

Suppose that two-thirds of the light coming from within the boundary of the farther screen be from the bright wires: then the phantom meshes will be three times as bright as the phantom bars; but at their edges they may blend into one another, the eclipses there being less complete. Thus no lines appear in the phantom whose pictures on the retina are not much broader than the picture of a point, even when out of focus, and hence the phantom is seen by near-sighted and far-sighted alike.

Phantoms often less simple and conspicuous may be got when the visual angles subtended by single spaces in the two screens are not approximately equal, but are approximately in a simple numerical ratio. The screens may also be of lattice-work, or pale fences, not necessarily parallel, seen two or three deep against the sky; and the effects are sometimes very beautiful.

Undoubtedly, when the screens are fine, binocular vision, with the stereoscopic matching of patterns, comes in, as suggested by Professor LeConte; making the phantom seem real and solid, and fixing its assumed distance from the observer. But I leave this part of the discussion to him, because he can treat it far better than I can. JAMES EDWARD OLIVER.

Cornell university, April 8.

I was gratified to find that the phenomenon described in No. 57 proved of interest to Professor Joseph LeConte. He states that my explanation of the cause of the phenomenon is erroneous, and I am in no wise qualified to dispute him. Nevertheless, a careful repetition of the experiment would indicate that his explanation is not the correct one. The phantom image is as readily seen with one eye as with two; and I still think I am correct in saying it is inverted and magnified. I hope Professor LeConte will make the experiment himself, and give us his explanation of the phenomenon. In the mean time, allow me to state the facts as they occurred in an experiment made after reading his letter.

Standing about twelve feet from an ordinary fly-screen, and looking through it at the blinds of a house about one hundred and fifty feet distant, phantom lines, alternately a light one and a dark one, are seen crossing so much of the field of view in which the blinds lie, but not continued beyond their limits. The lines remain visible, although one eye be closed.

The image rises as I bow my head, and sinks as I lift it. Is not this evidence of inversion?

I can readily count the lines that lie across a blind, twelve light and twelve dark ones; but, in order to correctly count the actual slats in the blind, I am obliged, on account of the distance, to have recourse to a telescope. My wife, who is short-sighted, can only distinguish the mere outline of the actual blind; but the phantom lines are plainly visible to her. The number of slats in a blind is thirty, which would give sixty alternating dark and light lines. Is not this evidence of magnification? F. J. S.

ANDREW ATKINSON HUMPHREYS.

American science is again called to mourn the loss of one of its leaders, the friend and colleague of Bache and Henry. With him, as with them, administrative duties restricted and hampered individual investigation; but he was able to accomplish enough in this field for lasting fame.

Andrew Atkinson Humphreys was born in Philadelphia on Nov. 2, 1810. His family was one of the oldest and most distinguished of Pennsylvania, represented in the first continental congress, and eminent for two generations in the corps of naval constructors. To the skill of his grandfather were due the designs on which were built the famous Constitution and her five sister-frigates, which carried the flag of the Republic so proudly in the war of 1812.

When between sixteen and seventeen years of age, the boy entered the military academy at West Point, then almost the only mathematical and scientific school in the country. He was graduated in 1831, in the same class with Henry Clay, jun., who fell so gallantly in the battle of Buena Vista; Professor Norton, late of Yale college; and several others eminent both in war and in peace. He first served in Florida as an officer in the second artillery; but the climate so affected his health, that in 1836 he was forced to resign his commission.

Two years later the corps of topographical engineers was re-organized as a distinct branch of the army, and Humphreys was appointed one of the first lieutenants. Among his earlier duties was to prepare a plan for extending and remodelling the Capitol at Washington; and his design, in many of its features, was finally adopted.

In 1844 he was assigned to the charge of the coast-survey office, under Professor Bache as superintendent; and for five years he labored most assiduously and successfully to perfect the organization of this institution. His assistance was appreciated by his chief, who always remained a warm personal friend.

In 1850 Capt. Humphreys was charged with the surveys and investigations, then inaugurated, to determine the best method of restraining the floods of the Mississippi River, and of deepening the channels at the mouths. This work continued for ten years, and, even if he had done nothing else for science, would have placed him at the head of his profession. The hydraulics of rivers have been studied by eminent physicists for hundreds of years: but it may safely be asserted that none among them displayed more skill in conducting investigations, or more ability in discussing results; while the size of the river, and the thoroughness with which the work was executed, were without precedent in any former operations of like character.

But this labor represented only a small part of the professional burden resting on Capt. Humphreys during those ten years of his life. He was also charged (1854) with the direction of the surveys for selecting the best railroad-route from the Mississippi River to the Pacific Ocean, and with discussing and analyzing the results, — a work which he accomplished in a manner scarcely less admirable than that upon the Mississippi. He was also an active member of the Light-house board and of several important commissions. Indeed, it seemed to those associated with him that there was no limit to the demands to be made upon him, or to his ability to meet them. His health suffered from his intense application, but was quite restored during the war.

Gen. Humphreys' military services need not be recounted here. They were of a distinguished character, especially after Gettysburg. From that date he was either chief of staff of the Army of the Potomac, or in command of one of its army corps; and his brilliant reputation as a scientific man was equalled by that acquired as a soldier.

Shortly after the war he was called to the chief command of the corps of engineers and of the engineer department, with the rank of brigadier-general; and for thirteen years he filled this responsible position in a manner to win the respect of every one thrown in contact with him. In 1879 his name was placed upon the retired list of the army at his own request.

With such a record, and at the age of over threescore years and ten, most persons would be content to rest on their laurels. Not so was Gen. Humphreys. His connection with the Army of the Potomac had been of a character to render him, of all men, the most fit to write its history. He undertook the task for the period after Gettysburg; and in two volumes of the 'Campaigns of the civil war,' published by Scribner, he has left a military classic which will form the basis of future history. It is to be regretted that the limits as to size, of this publication, rendered a degree of condensation necessary which has marred the work for any but a professional reader.

Gen. Humphreys' individual contributions to science, and his care to advance its interests,

were appreciated. In 1857 he was elected a member of the American philosophical society; in 1862, an honorary member of the Imperial royal geological institute of Vienna; in 1863, a fellow of the American academy of arts and sciences. In the same year his name was placed on the list of the original corporators of the National academy of sciences. In 1864 he was elected an honorary member of the Royal institute of science and arts of Lombardy, Milan. He was also a corresponding member of the Geographical society of Paris, of the Austrian society of engineer architects, and of the New Orleans academy of sciences. In 1880 he was elected an honorary member of the Italian geological society. The degree of LL.D. was conferred upon him by Harvard college in 1868.

In the regular service, beside the ordinary promotion in his corps, he received the brevets of colonel, brigadier-general, and major-general, for gallant and meritorious services in the battles of Fredericksburg, Gettysburg, and Sailor's Creek.

Sincerely your friend
AA Humphreys

Gen. Humphreys' death occurred at his home on the evening of Dec. 27, 1883. He passed away when reading at his table, shortly after being left, apparently in good health, by the family. The manner was what the soldier himself would have chosen.

It is difficult, in referring to the personal character of Gen. Humphreys, to avoid seeming exaggeration. He united to all the manly virtues the delicacy and refinement of the weaker sex. Any thing mean, cowardly, or Jesuitical excited his indignation; and the higher the position of the culprit, the more certain and violent the explosion. He was generous in the highest sense of the word, and slow to suspect evil. His mind was analytical, original, and inventive. His intuitive perceptions were of surprising accuracy, but he held his judgment in reserve until the evidence was presented and weighed; then he took his position, and could not be moved. It was impossible to be thrown into intimate relations with him without being impressed with his strength, and charmed with his lovable character.

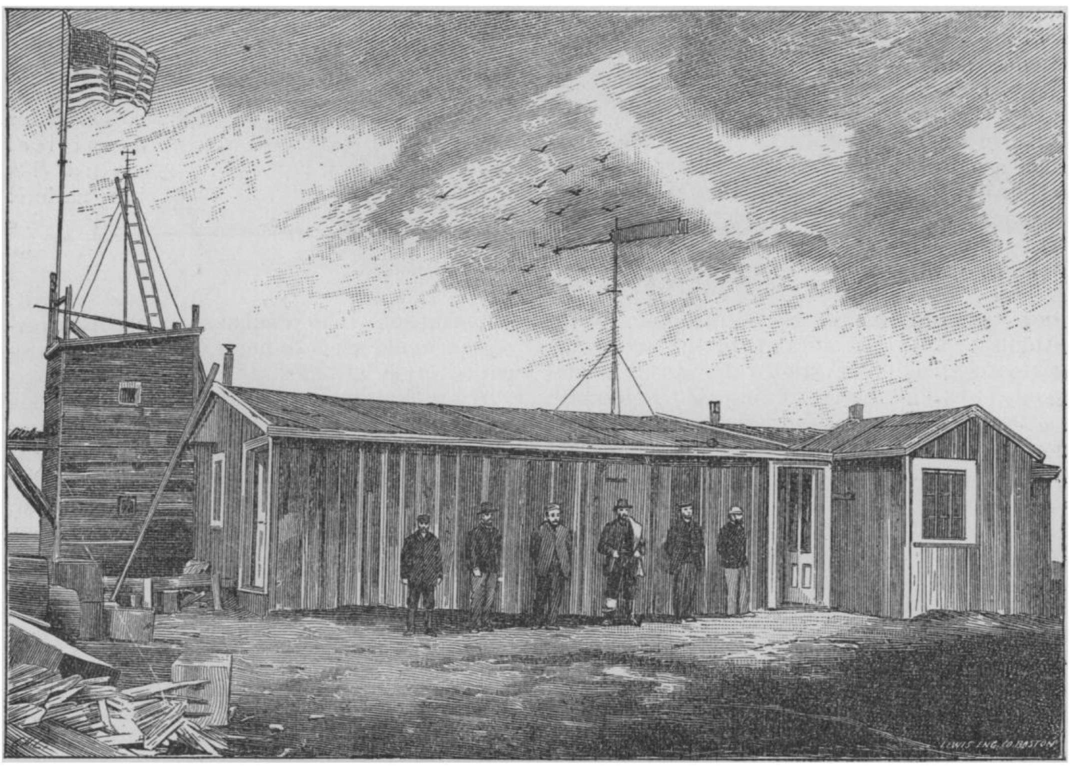
HENRY L. ABBOT.

THE U.S. METEOROLOGICAL STATION AT POINT BARROW.[1]

The U. S. expedition to Point Barrow, Alaska, sent out under the auspices of the Signal-service in 1881, was one of the International polar expeditions determined upon by the International polar congress, which met in Hamburg in October, 1879, and was designed to co-operate with the various stations established around the north pole in simultaneous observations in the three elements of magnetism and in meteorology. Nearly the whole civilized world was represented in this work; and, commencing with the Greely party at Lady Franklin Bay, they posted their chain of videttes around the pole in the following order: Denmark, Upernavik and Godthaab; Germany, Pendulum Island; Austria, Iceland and the Island of Jan Mayen; Sweden, Mosel Bay, on West Spitzbergen; Norway, Bossekop; Finland, Sodankyla; Russia, Moller Bay, on Spitzbergen, and at the mouth of the Lena; Holland, Dickson Haven; United States, Point Barrow and Lady Franklin Bay. The series of international observations proper was to commence Aug. 1, 1882, and end Aug. 31, 1883.

The little colony for Point Barrow, consisting of ten persons in all, sailed from San Francisco, Cal., in the schooner Golden Fleece, on the 18th of July, 1881, and, after a long, monotonous voyage across the North Pacific, passed into Bering Sea, through the Unimak Pass, on the 15th of August, and after touching at Plover Bay, Siberia, to correct the rate of their chronometers by observation on that well-established meridian, passed through Bering's Strait on the twenty-seventh day of August, and reached their destination on the 8th of September, fifty-one days out from San Francisco. The vessel returned to the United States at once, after discharging her cargo.

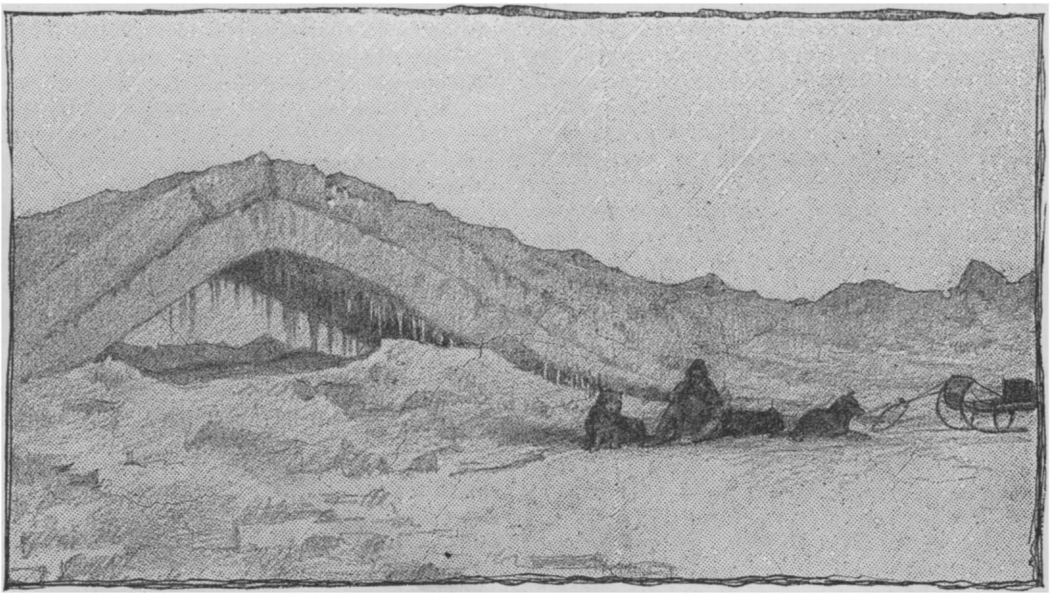
The energies of the party were at first entirely devoted to housing themselves for the winter, and securing their stores; and the 1st of October found them with the building nearly completed, observatories up, and with three years' supplies well secured, in a condition to



SIGNAL-STATION AT POINT BARROW, ALASKA UNITED-STATES.

[1] Communicated by permission of Gen. W. B. Hazen, chief signal-officer.

look the future very confidently in the face. The instruments were placed, and hourly observations in meteorology commenced, on Oct. 17, and in magnetism on Dec. 1; and this work was carried on without a single interruption from that time up to the last hour the station was occupied, on the twenty-ninth day of August, 1883. In addition to the prescribed meteorological observations, the work was extended to taking and recording the temperature of the sea-water, top and bottom, the sea-ice, and the earth, and measuring the terrestrial and solar radiation.



ICE-ARCH FORMED NEAR POINT BARROW.

The auroral display of this region was grand beyond all description; and nearly every unclouded night from September to May was lit up with the dancing light of this strange phenomenon; and, when shut from our view by the clouds, the disturbed needle told of its presence.

The tidal record shows that the adjacent ocean is almost tideless; and that there is no influx of warm water from the Japan current or elsewhere was shown in the even temperature of the sea-water at all depths, from October to June.

The earth was found to be frozen to an immense depth. The temperature at thirty-eight feet (the lowest depth attained) was + 12° F., and the ratio of increase of temperature for that distance gives the depth of the frost to be nearly three hundred feet. At the lower depths attained, the temperature never changes; and at the depth of twenty-six feet, which was fourteen feet below the sea-level, was found a pair of wooden snow-goggles of the same pattern as those worn now, showing that this region has been inhabited by man for a longer period than has generally been supposed by ethnologists. The peculiar race inhabiting this region would seem to be indigenous to the ice period in all latitudes.

After November we found that all animal life disappears from the land; and, except for an occasional stray reindeer or white fox, we saw no living thing but the Innuit and his dog from November until May, and the sea gave us only the arctic cod and small hair seal (Phoca foetida).

Lying between us and the pole was a sea of eternal ice, which we believe is an impassable barrier between us and latitude 90° north: over its rough and broken surface it is impossible to travel with any known means of transportation. Unencumbered and on foot is the only manner in which man can penetrate this unknown world: so the distance he can travel is limited to the number of days he can carry supplies on his back, or by the slow process of floating; and nature is ever crowding him back, even where the current sets to the north in her process of restoring her equilibrium at the pole. We found from long, careful observations, that the maximum thickness of sea-ice over still

water was about seven feet; but, owing to the influence of gales and currents, the whole ocean is filled with pack from fifty to two hundred feet in thickness. During the whole winter this region is subject to violent local gales, which open huge cracks in this frozen ocean, often extending many miles, and from ten to five hundred yards wide. These cracks freeze over very rapidly, as the temperature of the sea-water alway stands at 29.2° F. We have known two feet and a half of ice to be formed over one of these cracks in ten hours. This expansion, acting like a great wedge, shoves the great masses of pack apart; and it can only find room in the direction of the lower latitudes, or side of the least resistance. Hence we see along the southern edge of the eternal pack a continual crowding-down of old ice, which has yielded to the new ice formed in the cracks, and, in its turn, is packed and displaced; but we never found that there was any accumulation of new ice on the submerged masses of old pack, no matter to what depth they rested in the water. This process going on daily and hourly, the ice over the pole is kept at an even thickness; the old, heavy ice, often high above the surface, yielding to the new. We never found that ice formed on the bottom of the sea, the lakes, or the rivers.

The migration of the eider occurs in May; and the flight is to the north-east, in the direction of Prince Patrick's Land. We never saw any flight of birds to or from true north at any season of the year. They commence returning along the western shore in July, and linger as long as there is any open water.

The members of the expedition found time to make a large collection in ethnology and natural history, which has been turned over to the Smithsonian institution, and is now being catalogued and placed. All records were kept in duplicate, and both copies were brought safely back to the United States. The official report is now being compiled at Washington, and will be issued by the signal-office as soon as published.

The party returned to the United States on the schooner Leo, chartered for that purpose; leaving the station Aug. 29, 1883, and returned *viâ* Bering Strait and the Pass of Akutan, landing at San Francisco, Cal., Oct. 7, 1883; touching only at St. Michael's, where Lieut. Schwatka and his party were found waiting for a chance to return to the United States, after their adventurous ride of over two thousand miles down the Yukon on a raft, and at Unalaska, to repair our little vessel, which had been damaged by the ice. P. H. RAY.

ON THE STATE OF THE INTERIOR OF THE EARTH.

THE appearance of the new edition of Thomson and Tait's treatise on natural philosophy affords an opportunity for geologists to object to conclusions reached by physicists in relation to the condition of the interior of the earth. Various physicists, chief among whom are Hopkins, Sir William Thomson, and G. H. Darwin, have concluded, from a discussion of the phenomena of precession and of tidal friction, that the earth is a solid, with a rigidity at least as great as steel. Some time ago Thomson retracted the first part of the argument, on a suggestion made by Newcomb, and the writer expected to see a retraction of the entire argument in the new edition of the philosophy; but it does not appear. Yet the statement is somewhat modified. On the 485th page of vol. i., part ii., the following paragraph appears: —

"These conclusions, drawn solely from a consideration of the necessary order of cooling and consolidation, according to Bischof's result as to the relative specific gravities of solid and of melted rock, are in perfect accordance with §§ 832–848, regarding the present condition of the earth's interior, — that it is not, as commonly supposed, all liquid within a thin solid crust of from 30 to 100 miles thick, but that it is, on the whole, more rigid, certainly, than a continuous solid globe of glass of the same diameter, and probably than one of steel."

It is not my purpose as a geologist to discuss the methods by which this conclusion is reached; nor shall I array the facts by which geologists arrive at a different conclusion. I propose simply to characterize the lines of inductive reasoning used by them. These are as follows: —

I. The argument from displacement.

The writer has carefully studied a fault in Utah and Arizona, about three hundred miles in length, with a throw varying from two thousand to five thousand feet. Everywhere along its course the displacement is easily seen: its verity is a fact of observation, confirmed by the observation of other geologists thoroughly competent. The fault is so plain, that the tyro in geology may see it. Now, in the case of this fault, three hypotheses may be entertained, — first, that the thrown side subsided; second, that the thrown side remained stationary in relation to the centre of the earth, and the opposite side was upheaved; or, third, that

while the thrown side went down, the opposite side went up. To explain the first hypothesis, we must have either a condensation of the mass underlying the thrown side, extending toward the centre of the earth, or a transferrence of the material from immediately beneath the thrown side to some other part of the interior of the earth. A condensation or a transferrence is absolutely necessary. The latter hypothesis is the hypothesis of a fluid interior. In the second case mentioned above, where the side opposite the thrown block is supposed to be upheaved, there must be either an expansion of the immediately underlying material, or a transferrence of material. The hypothesis of a vacuum beneath is untenable, for it can be easily demonstrated that the strength of materials could not possibly sustain the resultant stress. We are forced, therefore, to the conclusion that there is either an expansion or a transferrence of material, the latter being equivalent to the hypothesis of an interior fluid. The third case is, that one side went down while the other side went up; and this hypothesis is sustained by many concomitant facts. In this case there must either be a condensation on one side and an expansion on the other, or a transferrence of material: and geologists conclude that there has been a transferrence of material; i.e., they postulate a fluid interior.

Faults like the one above mentioned are not infrequent. Many are discovered of greater magnitude, many more of less; and, wherever the geology of any great district of country has been explored, such faults have been discovered, so that they are now known to exist in great numbers throughout all the studied portion of the land-surface of the earth. Every year's research — it may almost be said every month's or every week's research — adds to the number.

In addition to these faults, geologists are everywhere discovering flexures, many of them simple monoclinal bendings by which the crust of the earth is displaced; one side being lowered, or the other raised, or the two simultaneously moved. Again: great anticlinal flexures are discovered, sometimes developed to appressed folds, and monoclinal and anticlinal flexures are found throughout the whole known portion of the land-surface of the earth; so that these displacements by faulting and by flexure are widely and generally distributed.

Again: displacement is a phenomenon, not simply of the present time, but of all known geologic time; as like displacements are discovered of various ages, beginning with the oldest archaean, and extending to the present time. Nor can we say that displacement was either greater or less in earlier geologic times than in the present. Such displacements as have been here briefly characterized have occurred again and again in the same district of country, sometimes following the old lines, oftener along new lines, traversing in diverse ways the same territory; so that blocks of the crust that have at one time been upheaved have at another time subsided, and blocks that at one time have subsided have at another been upheaved. It is sometimes possible to discover as many as six or more epochs of alternating displacement; in the first the rocks going up, in the second down, then up, then down, etc. The evidence of repeated displacement in the same district is not simply local, but is widely spread throughout the known portion of the land-surface of the earth.

But the evidence for a fluid condition of the interior, derived from displacement, does not end here. Take first a simple example like the following: a block of stony crust is separated from the adjacent rock on all sides by fracture. Such a block may be many miles in length (ten or a hundred), and of varying width (two or twenty miles). Such a severed block will be found by the geologist to have careened, one side or edge going down while the other came up. In order to explain this displacement, it is necessary to assume that there was an increasing rate of expansion beneath the block from the axis of rotation to the upturned edge, and an increasing condensation of the underlying material from the same axis to the edge of the down-thrown side, or to assume that it careened on a fluid mass. The latter is the explanation accepted by geologists. Again: such a block may be broken into many parts, each one of which behaves as an integer, and careens on its own axis. Many such careened blocks have been discovered, though this particular form of displacement has not been described by geologists to the extent of those mentioned above.

II. The argument from vulcanism.

Fluid matter comes up from unknown depths, and is sometimes intercalated between horizontal or dipping beds of sedimentary rock; but oftener it comes to the surface and is poured out in sheets, sheet being piled on sheet until mountains and mountain systems are produced. The amount of matter thus brought up from below is great; and it occupies large

areas throughout all the known portion of the earth, forming the substance of many mesas, plateaus, and mountain systems, in which valleys and valley systems are carved. The pouring-out of this volcanic matter is not confined to the present time, or to late geologic time. Nor can the geologist assert that the rate of extravasation has increased or diminished from the earliest known geologic time to the present. It seems to have been paroxysmal by districts, but uniform, considering the whole extent of the surface of the earth. The magnitude of the volcanic formations exposed at the surface is such that the origin of the material cannot be attributed to local and trivial causes: it must be explained by laws of universal application. Extravasation is always associated, so far as the phenomena have been studied, with displacement; and this association is of such a nature that they must have a common explanation. This common explanation, as postulated by geologists, is a fluid interior.

III. The argument from internal temperature.

The hypothesis of a fluid interior is reached by another inductive method, — through the facts relating to increase of temperature from the surface downward. The rate of increase is not well known; it seems to be greatly variable. In general, it may be roughly stated, as it is by Thomson and Tait, as about one degree for each fifty feet; but in many cases the rate is much higher. Such an increase, known to extend so far down as observation and experiment have reached, if continued at the same rate, would soon give a temperature at which all known rocks would be melted; and the hypothesis of a fluid condition is thereby strengthened.

IV. The argument from the 'flow of solids.'

It is an hypothesis worthy of consideration, that pressure itself would reduce the interior of the earth to a fluid condition. That rigidity, which is the characteristic of the solid state, is due to molecular cohesion; but geologists everywhere in their researches discover that this molecular cohesion, or rigidity, may be overcome by pressure: for everywhere they find that rocks may be squeezed into new forms, bent, contorted, and implicated; that is, the force of compression existing in many thousands of feet of superincumbent rock overcomes molecular cohesion to such an extent as to cause rocks to yield (the molecular cohesion is broken down). Doubtless the element of time is involved, to some extent, as a rock may be bent with a small force, if sufficient time be allowed. But with increase of force there may be decrease of time; and the force engaged in compression, being the weight of miles of superincumbent rock, must be sufficient to greatly reduce the element of time, and perhaps to cause it to disappear. The last few years of experiment have added to the argument derived from geologic observation. Many solids have already been found to flow under pressure. The molecular constitution of solids is found to undergo a change by reason of pressure, so that new compounds may be formed thereby; and in pressure we have conditions for chemical change analogous to the conditions produced by melting. It is therefore an inductive hypothesis of the highest value, that all rocks may be reduced to a fluid condition — i.e., be caused to behave as bodies of minute parts, without rigidity of structure — by pressure alone.

The facts of observation and experiment characterized above are vastly multifarious and cumulative, and the conclusions in each case are strictly inductive. The theory reached by the consilience of these four inductive processes is so strong, that structural geologists are compelled to accept it, and contradictory conclusions are rejected. It therefore behooves the physicist to re-examine his data and his methods of logical procedure; for, perchance, he may discover that an error lurks therein. J. W. POWELL.

INERTIA.

RECENT conversations with teachers of physics have shown me that there exists, in this country at least, great diversity of opinion as to the proper definition and use of the term 'inertia.'

Elementary text-books usually speak of inertia as a mere *inability*, — the inability of a body to set itself in motion, or to stop itself when once in motion. This is an old use of the term, but certainly not the best use. Maxwell states,[1] that at the revival of science, "while the men of science understood by this term [the inertia of matter] the tendency of the body to persevere in its state of motion (or rest), and considered it a *measurable quantity* [the Italics are mine], those philosophers who were unacquainted with science understood inertia in its literal sense as a quality — mere want of activity, or laziness."

Maxwell suggests certain simple experiments

[1] Theory of heat, p. 86.

which the student may perform in order to become thoroughly acquainted with that property of matter which he calls inertia. I shall describe an additional experiment, for I find that the difficulty is not merely one of words. There are many people who do not recognize the physical facts to be dealt with.

Take a heavy weight, fifty pounds let us say, and suspend it by a long cord. To the weight thus hanging attach another cord, strong enough to sustain the fifty pounds. By means of this latter cord give a sharp horizontal pull to the weight. The cord is broken, while the weight hardly moves, — is left slightly swinging. Is it possible for any one to try this experiment, and not recognize that we have to do here with something more than the inability of matter to set itself in motion? Evidently we encountered a *resistance* in setting the body in motion. Whence came that resistance? Not from gravity: the pull was horizontal; and, moreover, the cord we have broken would have served to lift the weight. Assuredly not from friction, or resistance of the air. We are driven to the conclusion that matter possesses a property in virtue of which it *offers resistance* to an agency which is setting it in motion. We should find, too, by experiment, that matter offers a similar resistance when its motion is being changed in any way, either in magnitude or in direction. This property of matter, which is much more than the mere inability to set itself in motion, is what Maxwell, Thomson, and Tait call inertia.

Now, we must distinguish very carefully between inertia itself, a property of matter, and the resistance which matter can exert in virtue of that property, somewhat as we must distinguish between a man's strength (that is, the property in virtue of which he can exert force) and the force which he may be actually exerting at any time.

Returning to experiment, let us attach to our fifty-pound weight a weak thread, capable of sustaining a few ounces. Pull gently and steadily in a horizontal direction upon this thread. A resistance is felt, to be sure; but the weight is moved perceptibly in the direction of the pull, and acquires, perhaps, a greater velocity than we succeeded in giving to it by a pull which broke the cord previously used.

This experiment proves that the resistance which a given body can, in virtue of its inertia, offer to an agency which is setting it in motion (and it would be the same for any change in its motion), is a variable quantity — let us leave the statement unfinished for a moment, while we look for the conditions and the law of this variation. When the stout cord was broken in pulling at the hanging weight, the latter acquired a small velocity, it is true; but it acquired that velocity in a very short time, a fraction of a second. When pulled by the thread, the weight acquired a somewhat greater velocity, it may be; but a much longer time was occupied in the change. The exact quantitative law, which can be determined by experiment with such apparatus as, for instance, Atwood's machine, is expressed by completing the interrupted statement in the following words: — being proportional to the rate at which the agency is changing the body's motion.

This definite law being recognized, there should be an end of the current vague attempts at explaining such phenomena as, for example, that of a half-open door pierced by a cannon-ball without being shut. Text-books too frequently say, in such a connection, that "masses of matter receive motion gradually and surrender it gradually," or that "it requires time to impart motion to a body as a whole," — statements from which the student is in danger of getting the idea, if indeed he gets any idea, that the *time* is required in order to draw things taut within the body, and get its particles to acting upon each other, somewhat as it takes time and a succession of jerks to take up the slack of a freight-train while it is being started.

Let us note again that the resistance which has just been considered is not the body's inertia, but is merely the manifestation of that property. But if the manifestations of inertia, in the case of a given body, are so variable, how can we speak of inertia as a measurable quantity, as Maxwell does in the quotation already made from him?

Suppose we take a certain body, and ascertain what force, reckoned in any units we please, is required to impart to this body a certain velocity in a certain time. Then we take a second body, and ascertain what force is required to give it the same velocity in the same time. The second force may be equal to, less than, or greater than, the first. If the forces are equal, we may say that the two bodies have equal inertias. If the second force is n times the first, we may say that the second body has n times as much inertia as the first. This is comparison of inertias. If we wish for what is called *measurement*, we have merely to select some body, and agree to call its inertia the unit inertia.

E. H. HALL.

LOCALIZATION IN THE BRAIN.

Since 1870, when Fritsch and Hitzig showed that the cortex of the brain was excitable, physiologists have been actively experimenting on it. Thus far, investigation has given rise to two theories regarding the function of this gray matter. One theory looks upon it as the seat of the higher intellectual activities: the other, considering it as a sort of mosaic composed of small areas, looks upon each area as possessing some definite function, either sensory or motor. Moreover, such is the nature of these areas, according to this theory of localization, that, if they be stimulated, perfectly definite movements or sensations are excited, while, if they be destroyed, the movements or sensations over which they preside are abolished.

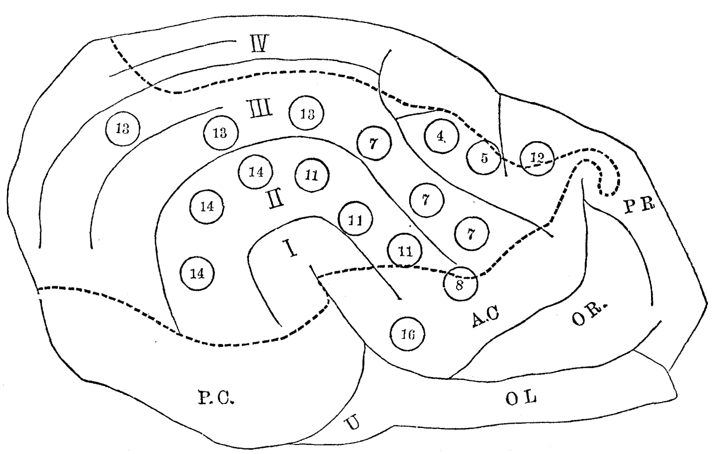
At the International medical congress held in London in the summer of 1881, these views were both ably represented; the former being supported by Professor Goltz of Strasburg, while Dr. Ferrier and Professor Yeo, both of London, represented the latter. Goltz had a dog, and Ferrier a monkey. The animals were exhibited before the physiological section of the congress, and each investigator stated his conclusions as based on the animals presented. Both animals were then killed, and their brains placed for examination in the hands of an eminent committee, consisting of Dr. Gowers, Dr. Klein, Prof. E. A. Schaefer, and Mr. J. N. Langley. The reports of this committee have recently been published in the *Journal of physiology* (vol. iv. Nos. 4 and 5); and, having now before us this complete description of the lesions, it is possible to estimate the value of the peculiarities exhibited by the animals while alive.

The dog presented by Goltz had been subjected, between November, 1880, and May, 1881, to five operations. By these a large portion of the cortex of both hemispheres had been washed away with a stream of water. Casually observed, this dog showed nothing abnormal in its bearing. It ran round the room, wagging its tail and sniffing about, as any dog is apt to do in a strange place. Its expression, however, was stupid, and its gait heavy; but it appeared to possess all its senses, and have control over all its muscles, — two points which are to be emphasized as of fundamental importance in the present discussion. This dog was, however, quite different in many ways from a normal dog. In travelling about, it avoided not only real objects obstructing its path, but those which were not real, — such, for instance, as a spot of sunlight on the floor, or a bit of white cloth spread flat.

Fear was apparently absent. The cracking of a whip and threatening gestures produced no effect; and, when an angry, spitting cat was held up to test the impression which it would make on this dog, it calmly began to lick the cat's face. It would eat dog's flesh, something which a normal dog is said not to do. When pent behind a low fence, it made no systematic effort to get out, although it apparently wanted to do so; the difficulty seeming to be that it did not know how.

Without further continuing the list of variations from the normal, it can be briefly said that this dog, though possessing his senses and not paralyzed, had yet lost something which goes to make up the difference between an intelligent and a stupid animal; or, to quote Goltz, there was a weakness of perception. The conclusions which Goltz drew from the actions of his dog are too obvious to need statement.

The monkey presented by Ferrier had been operated on seven months before. At that time what Ferrier calls the motor zone — a region about the fissure of Rolando — had been destroyed on the left side. This was done with a thermo-cautery. In the animal as exhibited, there was weakness in the right leg, and the position of the right arm was abnormal. No voluntary use of either of the limbs on that side had been noticed since the operation. Otherwise the monkey was quite well. Dr. Ferrier briefly stated that he considered this paralysis as due to the destruction of the motor zone of the cortex, which presided over the muscles on the right side of the body, the destruction of which would, according to the theory of localization, produce this effect. It now remained for the post-mortem to show what were the lesions in both these cases.



Right side of dog's brain, after Langley, slightly altered. I., first convolution; II., second convolution; III., third convolution; IV., fourth convolution; *A C*, anterior composite convolution; *P C*, posterior composite convolution; *O L*, olfactory lobe; *O R*, orbital lobe; *PR*, Prorean convolution; *U*, uncinate convolution. The circles numbered 4, 5, 7, 8, 11, 12, 13, 14, 16, represent areas localized by Ferrier on the brain of the dog, and have been taken from his fig 32, 'The functions of the brain,' p. 149. The broken line encloses the region which the lesion is known certainly to have covered, and within which all the gray matter of the cortex had been removed.

Mr. Langley, to whom the right brain of the dog

was given, has made a very thorough report. Indeed, he reviewed the whole subject of the fissures and convolutions of the dog's brain before giving his observations in this particular case. His main conclusions are perhaps best indicated by a figure showing the extent of the lesion. This is traced on a schematic outline representation of the right hemisphere seen from the side.

In some places doubt as to the exact extent of the lesion rose from the obliteration of some of the fissures, and a possible dragging of the brain during cicatrization. The region enclosed within the dotted line in the accompanying figure leaves out all the doubtful points, and includes the part only which is certainly known to be covered by the lesion.

In order plainly to indicate the significance of this injury, some areas localized for the dog's brain by Ferrier have been inserted in the figure in positions which are approximately correct. The number of these areas involved, as can be seen at a glance, is very large.

The left brain of the dog was examined by Dr. Klein. Without going into the details of his report, it may be stated that the extent of the lesion was rather less than that on the right side. The destruction of the gray matter did not extend quite so far forwards, nor so far towards the base, but it was still extensive enough to include some two-thirds of the areas which were embraced by the lesion on the opposite side.

If, then, the theory of localization were correct, we should have expected to find this dog largely paralyzed on both sides of his body, and blind in both eyes. That this was not the case, the actions of the animal plainly showed. There was some degeneration found in the deeper parts of the brain, but it was apparently of little importance.

The brain of the monkey was examined by Professor Schaefer. The lesion was found quite strictly confined to the motor zone. It thus covered an oval region, occupying about the middle third of the brain, and bisected transversely by the fissure of Rolando, the ends of which extend beyond the oval on both sides. Beneath this, in the medullary centre, was a secondary lesion having about the same extent. The basal ganglia were not involved. But the very important fact was developed, that the pyramidal tract connected with the left side of the brain had undergone Wallerian degeneration through its whole extent, while there was also found an unexplained tract of degeneration in the left lateral column of the cervical cord. These deep lesions being discovered, it became at once impossible to decide whether the effects observed in the monkey were due to a removal of a certain portion of the cortex or not; so that it cannot be considered that in this case the monkey presented by Ferrier furnishes any evidence in favor of localization. From the dog, on the other hand, which was exhibited by Goltz, the conclusion is warranted, that large portions of the cortex can be removed without producing any of those effects which would be expected if the theory of localization were true; and at the same time there is some reason to believe that the removal of portions of the cortex diminishes general intelligence.

We have discussed but two experiments, and they in themselves are not sufficient ground for any generalization; yet the position in the scientific world, of all concerned, is such as to render these particular observations of more than usual importance in the history of this interesting question, and hence worth some passing attention.

HENRY H. DONALDSON.

THE WINTER OF 1879-80 IN EUROPE.

THE meteorological conditions which characterized this phenomenally cold winter have been carefully studied by M. Teisserenc de Bort. There are but few as severe winters in a century, while the month of December was the coldest on record at Paris. This exceptional cold was due to, 1°, the position of the maximum pressure; 2°, the clearness of the sky; 3°, the presence of snow upon the ground; 4°, the calm which prevailed. These conditions were united for twenty-seven consecutive days. Proceeding from the characteristics of this particular season, the author discusses the subject of the persistence of areas of high and low pressure in certain localities, and the resulting weather phenomena. These 'centres of atmospheric action' destroy the parallelism of isobars and isotherms with the equator, and control the prevailing winds. Thus, an area of high pressure generally prevails in Siberia in winter, and a similar area at about 35° north latitude in the Atlantic, near Madeira. The displacement of these maxima produces modifications in the weather of the whole of Europe, causing these abnormal seasons. Three types of cold weather may be recognized: 1°, that characterized by the displacement of the Asiatic maximum towards Europe, in which the weather is dry and quite cold; 2°, that characterized by the removal of the Madeira maximum towards France and Europe, with low areas in Tobolsk and near the Azores, in which cold and calm weather prevail; 3°, that characterized by the displacement of the Madeira maximum northward, with relatively low pressures over central Europe and the Mediterranean, and giving rise to cold with dampness and snowfall.

Similarly, two types of mild weather may be noted: 1°, that characterized by low pressures in northern Europe, with the displacement of the Madeira high area towards Spain and the Mediterranean; 2°, that characterized by a general spreading of high pressure eastward to its maximum in Russia. These types are hardly distinct enough to be classed separately: both are accompanied by south-west or west winds, bringing warm and moist air from the ocean. While the fact of the controlling influence of barometric areas is fully recognized, it is not so easy to account for the displacements which are observed. The author supposes that these are due to changes in the thermic condition of different regions of the globe, but does not attempt to further investigate this subject. If it were possible to foretell the barometric conditions of

coming months, the problem of forecasting the character of a season would be capable of solution.

W. U.

OLIVINE ROCKS OF NORTH CAROLINA.

MUCH interest was attracted a number of years ago to the olivine rocks of North Carolina by the excellent paper of Dr. Genth, on 'Corundum and its alterations.' These rocks are also well known practically from their association with the mica and corundum mines of that state: hence any thing tending to elucidate the origin and history of these immense masses of olivine is of value. There has been recently published, in the Proceedings of the Boston society of natural history, a paper by Dr. Alexis A. Julien on these olivine rocks, which is of great value, even if some exceptions may be taken to his conclusions. The particular variety of olivine rock in North Carolina is designated as dunite; it having been named from Mount Dun in New Zealand, from which locality rock of this character was first described. In North Carolina the rock is found in oval or lenticular masses in a hornblendic gneiss; and a 'marked slaty lamination' is looked upon by Dr. Julien as stratification which dips at a steep angle. His reasons for regarding this banded structure as bedding-planes are, that, on microscopic study of thin slices, there is seen an alternation of coarser and finer irregular grains. Again: grains of chromic iron are found not only dispersed throughout the rock-mass, but also in thin bands alternating with the olivine bands. He found, however, a sharp break between the lamination of the olivine rock and the foliation of the hornblendic gneiss surrounding it. Again: when there has not been formed in the rock some material, of later date than the time the rock came into place, which serves as a cement to hold the olivine grains together, the rock is pulverulent and friable, like a loosely consolidated sand.

From the above, Dr. Julien draws the conclusion that this dunite is neither of chemical nor of eruptive origin, but rather an accumulation of *débris* from some older olivine rock of eruptive origin; that is, it is an olivine sandstone. The chief defect in Dr. Julien's reasoning is, that all the evidence which he gives in support of this view could exist equally well if the rock had some entirely different origin. In order to prove that any thing must have been formed in any particular way, we ought to seek for certain characters in it which could have been produced in that way alone.

Messrs. W. C. Kerr and C. D. Smith, who have spent much time in studying this olivine rock in the field, declare in favor of the eruptive origin of it; but they have published little or none of the evidence upon which their conclusion rests, and therefore one cannot judge as to its correctness. Every rock carries within itself, or in its relations to others, the story of its origin and subsequent history with more or less completeness. The correct reading of that story depends upon our skill and knowledge. If a rock is deposited in the hollows of another as a beach formation, it is easy to see that the effect it produces upon the boundary-rock is different from its action upon them as a lava-flow or an intrusive mass. So the last two cases present different relations, according to their origin, to the surrounding rocks. As a rule, it can hardly be considered safe to positively declare what the origin of an old crystalline rock is, until these relations have been carefully ascertained; and in this direction Dr. Julien's work is defective. The present writer's microscopic study of the North Carolina dunite showed him that the rock he was studying, even when destitute of some cementing-material, was not friable and pulverulent, while the sections to his mind presented characters belonging to eruptive rocks only. The olivine grains are separated by fine fissures, but every irregularity in the outline of one is matched by a corresponding irregularity in the adjacent bounding-grains. If these grains had been water or wind worn olivine sand, no such matching of the parts would have been possible. This any one can readily see for himself if he will examine any conglomerate, and observe the amount of interstitial material it takes to hold together and fit the pebbles to one another. Then let him remember that a sandstone is a conglomerate on a small scale, and, under the microscope, a conglomerate to the eye as much as the other is to the unaided vision. The olivine rock now under consideration has absolutely no interstitial spaces and no binding-material, but the grains are fissured and separated the same as the adjacent portions are separated in cracked and fissured glass. From this the conclusion naturally follows, that such structure indicates that these olivine grains were formed by the cracking of an olivine mass during the process of solidification, crystallization, and cooling; that is, from an eruptive mass.

Further, individuals of olivine are seen in polarized light to be made up of a number of distinct grains, as much separated by fissures from one another as the distinct individual grains are elsewhere in the section. This is a natural and common occurrence in an eruptive rock, but in a sedimentary one the parts ought to be scattered. Many of these individuals, too, are long, wedge-shaped masses with sharply pointed ends. If they had been water or wind worn grains they ought to have had these sharp edges worn, rounded, and broken. These long, lenticular, fissured individuals are also arranged at every angle to one another, when, if the rock were sedimentary, they ought to lie nearly parallel, and on their sides.

The alterations of the dunite described by Dr. Julien are important and interesting because they give rise to veins and other rocks. The corundum in these veins is looked upon as a secondary product, and not, as Dr. Genth held, the primary material from which many rocks originated. The change of the olivine rock to different rocks leads to the production of chalcedonic or cherty forms, hornblendic schists, talc schists, serpentine, etc. The change to serpentine comprises every variation, "from that in which the serpentine is diffused among the olivine

granules, merely as a minute fibrous network, or as films enveloping olivine cores, to that in which only minute particles of olivine survive as the nuclei of the granules, and to the final result of a true and complete serpentine."

Dr. Julien further claims, that the actinolites, amphibolites, hornblende schists, and many of the talc schists, steatites, and serpentines of the Appalachian belt, are the equivalents of the dunite of North Carolina.

The objections to some of Dr. Julien's views have not been offered from any spirit of criticism of his truly excellent paper, but for the purpose of causing a more thorough study of the field-relations of this rock, and a presentation of the evidence that study affords. If the evidence, then, sustains Dr. Julien's conclusions, his views will be accepted unhesitatingly. He has, indeed, given more evidence for his opinions than most writers on crystalline rocks are inclined to offer; for, as a rule, they appear to consider their mere dictum sufficient to prove the origin of any rock. It would seem that the time has come when statements regarding the origin of crystalline rocks cannot be accepted from *any* observer, unless these claims are accompanied by full and decisive proof of their correctness. To bring about this healthy state in the study of the North-American rocks, the present writer has labored for years, and will continue to labor.

M. E. WADSWORTH.

ABOUT GREAT TELESCOPES.

DR. RALPH COPELAND of Dun Echt, near Aberdeen, when returning to Scotland by way of this country a few months since, made a tour of several North-American observatories; and in a late number of *Copernicus* he contributes a paper on the Dudley observatory at Albany, the Litchfield observatory of Hamilton college at Clinton, the Warner observatory at Rochester, the Toronto observatory (Canada), the McGill college observatory (Montreal), the Harvard college observatory (Cambridge), the Winchester observatory of Yale college (New Haven), the two observatories at Princeton, and the U. S. naval observatory (Washington). The noteworthy portions of the equipment of these establishments are briefly dealt with, and the work generally specified on which they are employed. Dr. Copeland, having enjoyed the good fortune of seeing through a number of the finest telescopes in all parts of the world, places on record, at the conclusion of the paper, his general impressions of the actual state of telescope-construction on both sides of the Atlantic.

First as regards their optical merits: it does not seem to him that any material difference as to the mere power of separating close double stars exists in the object-glasses made by the chief opticians in Europe and America. On a night of good definition, any of their telescopes may be trusted to divide a fairly equal pair of stars at a distance indicated by Dawes's table,[1] of which the following is a sufficient specimen: —

Aperture in inches.	Least separable distance.
1.0	4.56″
2.0	2.28
3.0	1.52
4.0	1.14
6.0	0.76
8.0	0.57
10.0	0.46
12.0	0.380
15.0	0.304
20.0	0.228
25.0	0.182
26.0	0.175
27.0	0.169
30.0	0.152

We thus see that in this respect our telescopes are practically perfect, and also that the atmosphere on the very best nights is sufficiently steady to permit their full power to be used. If, however, we test them on double stars, of which the components differ very much in brilliancy, then it is by no means so easy to come to a certain conclusion. There is the secondary spectrum to contend with, respecting the character of which it may be said that a certain degree of personal taste or fashion exists. Some persons, notably opticians, seem to be little disturbed by a decidedly blue glare, while others prefer a wine-colored fringe. Perhaps, indeed probably, there is a physiological difference in the observers; for, if we suppose a person to be blind to the extreme blue and the violet rays only of the spectrum, to him an over-corrected object-glass would be perfect. With it he would be able to make out the closest companions of blue stars, or to see comparatively faint ones right up to the moon's bright limb. To such a person, however, an object-glass under-corrected to the same extent would appear to be a decidedly bad one. To Dr. Copeland, as well as to many of his colleagues, an average glass by Cooke or Grubb, and, to a less extent, by Clark, appears over-corrected; while one by Schröder, and some of the Munich glasses, appear under-corrected. But here an important practical difference enters into consideration, one which has been particularly experimented on by Mr. Russell of Sydney; viz., that the correction of an object-glass may be lessened by separating the lenses: so that an over-corrected object-glass may be adjusted to any desired extent, while one that is under-corrected can only be used in the state in which it left the maker's hands. As an example, it may be mentioned that the somewhat over-corrected object-glass of the 15-inch equatorial at Dun Echt has been materially improved by separating its lenses 0.2 of an inch, while a separation of 0.3 of an inch was found to throw out too much red about the primary image. This degree of improvement is best shown by the extremely linear character of the spectra of stars which it now gives. But in this connection it is only fair to mention, that, in making this object-glass, Mr. Grubb was limited to the relatively short ratio of 12 to 1 between the focal length and aperture. Opticians have not neglected to avail themselves of this property; and

[1] Mem. roy. astr. soc., xxxv. 158.

accordingly we find three of the largest objectives in the world, — the 27-inch at Vienna, by Grubb; the 23-inch at Princeton, and the great Russian 30-inch, both by Alvan Clark & Sons, with their lenses separated by a considerable interval.

Assuming a large lens to be made of satisfactorily good disks, and having its curves and interval so adjusted as to give the best attainable results, there is another detail of construction which demands increased attention with every augmentation of size; i.e., the state of the surfaces of the lenses. Formerly it was too readily assumed, that, provided the curves were right, a few scratches more or less did not matter. There is a well-known story of an optician, who, on being blamed for turning out a badly scratched lens, replied that an object-glass was to be looked through, and not at. The optician was nevertheless in the wrong; for if delicate objects are under examination, no matter whether they are small companions of large stars, or minute satellites of bright planets, there can be no doubt that the finish of the objective plays a considerable part in their visibility. Nor is it merely necessary that the surfaces should be correctly formed and well polished: it is also requisite that they should be kept scrupulously clean, and, above all, free from grease, the slightest trace of which, when spread over a lens, must throw out irregular diffraction spectra, materially affecting the visibility of any small point of light in the neighborhood of a brilliant object. In this respect no practical astronomer should neglect to assure himself that an object-glass is really doing full justice to the maker.

Dr. Copeland's remarks on the mountings of large equatorials are especially pertinent. In America, he says, the mounting is just or barely sufficient to permit of a satisfactory use of the grand optical powers of their larger instruments; and no refined detail of auxiliary apparatus is attempted. On the continent we find the convenience of the astronomer studied in the most painstaking manner, and perhaps in no instruments in the world is this so carefully kept in view as in the finer German instruments. This is doubtless due in no small measure to the intimate relations which exist between the chief continental instrument-makers and practical astronomers; so that just that kind of apparatus is provided which experience has shown to be requisite. On the other hand, in the stability and rigidity of their mountings, the larger English and Irish instruments stand preeminent, while they year by year show a greater variety of really available subsidiary apparatus. Indeed, there can be little room for doubt that the elder Grubb, by his elegant arrangements for relieving the friction of both axes of the equatorial mounting, practically removed all limits to its size and strength; while in the little-known 25-inch refractor at Gateshead, by Cooke & Sons, we have a telescope which only requires to be efficiently used in a good atmosphere to show its great merits in all respects.

Finally, Dr. Copeland thinks, that whether we take large European or American instruments, the prospect is most encouraging, both to the astronomer and the instrument-maker. Nowhere can signs be detected that the utmost practical limit has been reached. A 27-inch glass can be managed with probably greater facility now than a 10-inch fifty years ago, and with something closely approaching to the full gain in power, due to increased size. The question of size now, as it did then, reduces itself to the production of suitable disks of glass and to cost. Here it is that silvered-glass reflectors offer facilities of which several distinguished investigators have not been slow to avail themselves.

ENTOMOGRAPHY OF HIRMONEURA.

Dr. Friedrich Brauer has, during the past season, been able to add considerably to our knowledge of the life-history of the Hirmoneura obscura, and the results of his observations have been published (*Sitzungsb. akad. wiss. Wien*, p. 865). During the latter part of June he found within the nearly formed pupa of Rhizotrogus the second larval stage of the Hirmoneura, which resembles the first stage in the structure of the mouth-parts (see *Science*, No. 12), but lacks the pseudopods and ambulatorial filaments so characteristic of that stage. How and when the young Hirmoneura larva gets at the Rhizotrogus larva still remains unknown; but Brauer assumes (and I think he is quite safe in doing so) that it enters the larva (not the pupa) of the Rhizotrogus, and is a true parasite, and not merely a predaceous insect. Having entered the Rhizotrogus larva, it seems highly probable that the Hirmoneura larva has to undergo a kind of quiescent larval state of uncertain duration, but which suddenly changes to one of rapid development during the pupal state of the beetle, which lasts only from two to three weeks. Hirmoneura larvae in the second stage, of about eleven millimetres in length, were found in Rhizotrogus pupae; and ten days afterward the full-grown parasitic larva, twenty-two millimetres in length, was found. Brauer thinks it more than probable that the full-grown Hirmoneura larva, after emerging from the Rhizotrogus pupa, hibernates; the perfect fly appearing in July of the next year. This seems to me more doubtful. The Rhizotrogus larva is known to require two years for development. There are two alternatives for the Hirmoneura larva: either it is carried, by clinging to the beetle, into the ground, and remains quiescent, either attached to or near the Rhizotrogus larva, for nearly two years; or it is capable of independently discovering the Rhizotrogus larva when this last is in its second year's growth. The first seems to me the most probable, and would give two years for the development of the Hirmoneura, or even three if the full-grown larva hibernates. In either case, the young Hirmoneura larva is endowed with a sense which is truly marvellous, whether we choose to attribute to it consciousness of its acts, or ascribe them to 'blind instinct.'

Brauer raises a curious practical question, which would indicate that old pine fences or felled trees in a field may, in this particular case, serve to prevent the undue multiplication of the Rhizotrogus 'white grub.'

C. V. Riley.

THE BONE-CAVES OF POLAND.

The bone-caves of Ojcow in Poland. By Prof. Dr. Ferd. Römer. Translated by John Edward Lee, F.G.S., F.S.A. London, *Longmans*, 1884. 41 p., 14 pl. 4°.

Our knowledge of primitive man in Europe, during the paleolithic age, is mainly confined to what has been learned in regard to the life and habits of the so-called 'cave-dwellers.' This has been principally obtained from the scientific exploration of numerous caverns, mostly situated in western Europe, in France, Belgium, and England; although inhabited caves are not wanting in other countries, especially in Germany, Spain, and Italy. The farthest point to the south to which they have been traced, is Sicily and the extreme south-eastern promontory of Italy; in central Europe they are quite rare; while the most easterly part in which any similar discovery has been made is Russian Poland. There, in the neighborhood of the village of Ojcow (pronounced Oïzoff), some fifteen miles or more north of Cracow, several caves have been discovered within the last ten years in the Jurassic limestone, which forms the sides of some of the beautiful valleys, in which there have been found human remains associated with the bones of animals, both those extinct and those still living. In the year 1874 Count Johann Zawisza of Warsaw began to publish in the scientific journals of that city, in Polish and French, the results of his careful explorations of many of them; and the *Matériaux* for that year contained a *résumé* of two of these papers. At the prehistoric congress held in Stockholm, also in 1874, Count Zawisza gave a brief account of his work in a paper entitled 'The age of polished stone in Poland.' He again called attention to his discoveries at the next congress at Buda-Pesth in 1876, mainly in the one called 'The cave of the mammoth;' about which he has since published more in the memoirs of the Anthropological society of Paris in 1878 and 1879.

In the *Matériaux* for January, 1882, M. G. Ossowski has given an account of the researches undertaken by him in 1879, on behalf of the Academy of sciences of Cracow, in several caverns situated in the eastern part of the *arrondissement* of Cracow, especially in the valley of Mnikow. About twenty of them yielded objects of human fabrication, all belonging to the neolithic period. Those fashioned out of bone were the most remarkable, and these are figured in two plates.

This comprises all that had been given to the world, so far as we are aware, in regard to the caves of Poland, prior to the appearance of the present work at Breslau in 1883. In it Professor Römer has given quite an elaborate report upon the explorations carried on by himself in 1878 and 1879, more especially in the cave of Jerzmanowice; together with an account of the results of Count Zawisza's explorations of 'The cave of the mammoth,' and some slight notice of what has been discovered by other persons in six different caverns. It is evidently intended to serve as a complete monograph upon the Ojcow caves; and from the fact that it has been deemed worthy of an English dress by the accomplished translator of Dr. Keller's 'Lake-dwellings,' and the handsome manner in which it is printed and illustrated, we hoped to find in it a fit companion to such classic works of prehistoric archeology as Lartet and Christy's 'Reliquiae Aquatanicae,' Dupont's 'L'homme pendant les ages de la pierre,' and Boyd Dawkins's 'Cave-hunting.' We regret to be obliged to state that our expectations have been disappointed, and that we have found the work quite unsatisfactory, at least upon the archeological side. In the paleontological department, there is evidence of knowledge and experience, leaving little to desire; but it is plain that neither the author nor the translator has any clear and adequate comprehension of the distinction between the paleolithic and the neolithic ages. We find the statement on p. 41, that "the remains of the ancient inhabitants consist of implements of hammered flint (paleolithic Tr.)," etc.; while on p. 7 it is said that in the cave of Jerzmanowice "no polished flint tools were found. The flint implements all belong to the older stone age." Evidently, 'implements of hammered flint' and 'polished flint tools' are intended to be contrasted; but, if we turn to the plates, we find that all the objects represented are either flakes, knives, or scrapers, and not a single true paleolithic implement is either delineated in them or described in the text. Very different is Count Zawisza's careful statement, that, "of the fourteen caverns I have excavated, one only had been inhabited by quaternary man; three belonged to the age of polished stone; two had served for a habitation of cave-bears; and in the others I found nothing" (*Cong. of Stockholm*, p. 260). Again: "The deeper we dug, the larger became the implements of the Moustier type, or of those of the quaternary gravels of Mesvin" (*Matériaux*, vol. ix. p. 90). So, too, Dr. Römer evidently is not aware that pottery was unknown in paleolithic times; for in his account of the cave of Jerzmanowice,

in which he declares that "the implements all belong to the older stone age," he states that "numerous pieces of burnt pottery gave further evidence of the existence of man in the cave."

In brief, we miss any indication of the employment of the strictly scientific methods of conducting explorations, according to which the exact depth and position in which each object was found are noted, whether it was covered by a floor of stalagmite or not, and what articles were found together; and we have instead only a jumble of miscellaneous remarks, however interesting in themselves. The plates are beautifully executed, and are valuable, especially those in which the animal remains are delineated; but the half-dozen devoted to archeology represent nothing absolutely novel, although several important specimens are figured. A number of human skulls and bones have been found in the different caves, which have been submitted to Professor Virchow's examination; and an elaborate account is given of his careful study of them. He reports that he finds nothing to indicate a high antiquity for them, and no material differences from the form of skull of the present inhabitants of the country: in short, there is nothing to prove that they are not the result of intrusive burials, and consequently not of the same age as the implements occurring with them.

Two interesting facts we find mentioned: one is the enormous amount of the remains of the cave-bear, discovered by Dr. Römer in the cavern explored by him, amounting to as many as one thousand individuals; the other is the proof obtained of the co-existence of man and the cave-bear from the finding of a vertebra of the bear, and an undoubted flint implement, embedded side by side in the same solid crystalline stalagmite. It is evident, from the general result of the explorations, that the caves were inhabited almost exclusively in neolithic times; although Professor Römer thinks that the occupation continued into 'the bronze age.' But the fibula figured by him in proof of this is plainly Roman; and in one cavern, even glass beads were found at a considerable depth in the deposit. Complete evidence of the very late occupation of one cavern, at least, is afforded by the discovery in it of a *denarius* of Antoninus Pius, of the year 140. But there is nothing remarkable in this, as Roman coins have frequently been found in the neighboring province of Silesia; and a hoard of early Greek coins was recently dug up near Bromberg, in Posen, on the lower Vistula. Their presence is to be traced with the greatest probability to the traffic in amber, which has existed from the remotest antiquity, and for which the trade route lay directly up the valley of the Vistula to Königsberg, in whose neighborhood similar finds have occurred.

The author states in his preface, that he had "the determination of undertaking a thorough investigation of these caves," but that he regrets, that, with respect to "the specimens found, it cannot always be positively stated from which bed in the caves they were taken; but the same is the case with most of the caves which have been excavated in Germany." We can but regard such a statement as this as disgraceful to German science, if true; and it certainly is not true of cave-explorations in other countries.

ILLINOIS COAL-PRODUCTION.

Statistics of coal-production in Illinois, 1883: A supplemental report of the State bureau of labor statistics. JOHN S. LORD, secretary. Springfield, *Rokker pr.*, 1883. 144 p., 2 maps. 8°.

THIS report, published in advance of the regular biennial report of the bureau for 1884, makes quite a comprehensive showing in regard to the coal-production of the state, and demonstrates the increasing value of the industry. Since 1870 the output of coal in Illinois has increased from more than two and a half millions of tons to more than ten and a half.

In the introduction, Illinois is stated to have no equal, in the states west of Pennsylvania, in the extent of its coal-fields, the abundance and accessibility of its deposits, in its transportation facilities, or in its annual contribution to the fuel-supply of the country. As to the extent and accessibility of the coal-fields, and the facility of transportation, this statement is undoubtedly correct. Albert Williams, jun., in the 'Mineral resources of the United States,' estimates that the state contains a total of 28,845,000,000 tons of coal. The numerous railroads with good grades furnish cheap transportation, and in Chicago and St. Louis the requisites of two great central markets are found. As far as the production is concerned, Illinois is perhaps equalled by Ohio. The statistics of the latter for 1883 are not at hand, but the rate of increase is probably about the same in the two states. Mr. Joseph Nimmo, in the abstract of statistics for 1883 (published by the U. S. treasury department), gives the production of coal in Ohio for 1882

as 9,450,000 tons; while Mr. Lord, for the same year in Illinois, gives 9,115,653 tons. Coal is mined in forty-nine counties in Illinois; and the number of mines is 639, employing nearly 24,000 men and a capital of $10,396,540. The production was 10,508,791 tons for 1883, valued at $15,310,521. This was an increase of 1,393,414 tons over the output of 1882. The average value per ton of the coal at the mines has been $1.46 for the past three years. There has been a marked decline since 1870, when it was $2.32.

The report gives a statistical summary for the state; the complete statistics of each county arranged in alphabetical order; and a comparative table for 1882 and 1883, showing the number of men employed, the product in tons, and the average and aggregate values.

There are also papers on 'Miners' wages,' and 'Casualties in mines;' and a detailed description of 'the Diamond-mine disaster at Braidwood,' with a diagram of the mine, is given. The subject of state legislation in the interest of the miners is considered, and statistical tables of the various inspection districts are presented, illustrated by a map showing their boundaries. These are followed by a list of the railroads in the state on the lines of which coal is found, with the names of the towns and stations on each where it is mined and shipped.

The average wages received by the miners is stated to be ninety cents per ton. During the year, 365 casualties occurred, involving the loss of 134 lives. This was at the rate of one for every 78,424 tons of coal, or one man in every 146 employed under ground. The catastrophes at Braidwood and Coultersville, in which 79 lives were lost, of course swells the list, and makes it exceptional; but, leaving them out, one life was lost for every 192,887 tons of coal taken out, which is an excessive death-rate for mines as free from explosive gases as the mines of Illinois are. In the bituminous mining-region of Pennsylvania the average for 1882 was one death to every 277,124 tons of coal mined; and in Great Britain the statistics for eight years, ending with 1880, show that for every 143,667 tons of coal taken out there was an average of one death.

In the Illinois mines the larger number of the miscellaneous accidents are caused by the falling of the roof, against which, as the report says, the miners are usually able to protect themselves. Familiarity with the danger, however, leads them in many cases to neglect the setting of props. Twelve of the 365 accidents were due to gases.

The report concludes with an enumeration of the state mining-laws.

Although residents of Illinois will be especially interested in this report, there is a great deal of material in it that is of general interest and practical value.

CARPENTER'S ENERGY IN NATURE.

Energy in nature: six lectures upon the forces of nature and their mutual relations. By W. L. CARPENTER. London, *Cassell*, 1883. 15+212 p., illustr. 12°.

WHEN a man has been driving a butcher-wagon, or throwing trunks, or wading about in the cold and wet all day, and has no attractive fireside to retreat to in the evening, it must be comforting to find a well-warmed and brightly lighted hall standing open, with a platform at one end loaded with bright apparatus, and curiously colored diagrams on the walls.

The weary man walks in and takes his seat among a crowd of equally curious men, or only equally weary if *habitués*, and after rubbing his hands, and smoothing his hat across his knees, gives a few furtive glances at the lecture-table, and awaits events.

Over the uppermost diagram there is posted in the boldest letters, 'Energy in nature.' Our tired friend has a flickering thought that it might be well if there were no energy in nature. With Nature he was acquainted when a boy, possibly, and has a certain system of philosophy in regard to her workings. He once saw a man who could discover springs of good water by means of an apple-twig. He has leaned his head against telegraph-poles to hear the despatches, or has watched for them as they passed on the wire. He has always been taught that each 'new moon' is a new moon, and, to the best of man's knowledge, made of some common substance necessarily. He is not aware that any of these cherished notions are to be jarred this evening; and, thanks to soothing sleep, they may not be.

The lecturer appears, — a man well acquainted with the mechanical theory of heat, the kinetic theory of gases, the peculiarities of a magnetic field, and the working of an induction-balance, brilliant results of the labor of man, — and has come this evening to flash these jewels before the eyes of his motley audience.

The lecturer begins; and the listeners catch 'electricity,' 'heat,' 'sand,' 'wood.' Two close their eyes and nod (the 'regulars' have already closed their eyes and nodded). 'Energy is the power of doing work,' the lecturer

says: the sleeper opens an eye. 'Force is simply the expression of the rate or speed at which any change takes place in matter:' the eye closes.

The lecturer, building his hopes on the staring eyes of a young man in the front row and the rapidly running pencil of the young woman in the second, dilates upon the first two laws of motion, and approaches the third. He notices a frightened look in the young man's face, and that the pencil has stopped, and says, "Action and reaction are equal, but for present purposes it need not be here discussed."

It may be said that the book-binder's apprentice over the clock has been omitted from this account of the audience. That is very true; but it must be understood, when a popular lecture is given, that it passes right over the heads or through the heads of nearly all who are there; that the results are only to be found in the minds of a stray few. With this granted, one may acknowledge that the blue lights and red lights of the experiments may draw applause, but that the main result of the evening will be a restless sleep for the majority, and a pleasant pastime for a few.

With the fire of the experiments buried in the black and white of woodcuts, and the awakening influence of the speaker's voice gone, the same half-told facts appear weak when read from the pages of a book.

Mr. Carpenter states in his preface, that kind friends advised the publication of his lectures; but the lectures being of the class which hint at rather than discuss the problems of physics, and intended to lead the listener to think he is learning when he is only listening to pleasant chat, it would seem that this advice must have been of the kind which is not meant to be followed.

SOME STATE AGRICULTURAL EXPERIMENT-STATIONS.

Annual report of the Connecticut agricultural experiment-station for 1883. Printed by order of the legislature. New Haven, *Tuttle, Morehouse, & Taylor, pr.*, 1884. 120 p. 8°.

Fourth annual report of the New Jersey state agricultural experiment-station for the year 1883. Vineland, *Wilbur pr.*, 1883. 112 p. 8°.

THE report of the Connecticut station for 1883 presents a good illustration, both of the value of experiment-stations and of the rather narrow limits within which their activity has been in most cases thus far confined. This oldest of the American stations owed its origin to the demand for an efficient control of the quality of commercial fertilizers. It was in its inception, and has remained to a large extent, a fertilizer-control station; and this, not from any lack of interest in the problems of agricultural science, nor from any incompetence on the part of its officers to solve them, but simply from force of circumstances.

During the winter of 1882–83 the station was without laboratory facilities, and the present report covers about nine months of work. Of its hundred and twenty pages, about seventy are devoted to fertilizers, two hundred and nineteen analyses of which are reported. "Nearly one-half of them are samples of complex composition, each one requiring six determinations in duplicate." The amount of work which this involves can be fully appreciated only by a chemist, but its effect in limiting the amount of other work done is obvious.

Aside from fertilizer analyses, we find in this report numerous tests of the vitality of seeds, together with a description of a new and convenient form of apparatus for the same; analyses of feeding-stuffs, and a table of the composition of American feeding-stuffs compiled exclusively from American analyses by Dr. E. H. Jenkins; analyses of the milk of Ayrshire cows, and of market milk; analyses of oak and chesnut leaves at different periods of growth; and divers minor matters, including notes on some analytical processes.

It will be seen, that, while considerable work other than fertilizer analysis has been done, it is all, so far as reported, laboratory work. Of experiments with living plants or animals, or even with the soil from which they draw their sustenance, we find no mention. As we have already said, this fact is largely, if not entirely, the result of unavoidable circumstances. We mention it here, not to find fault, but to express the hope, that, with its new equipment and increased income, the Connecticut station will find means and opportunity to enlarge the scope of its work, and attack some of the numerous problems in what we might call applied biology, which are waiting solution.

The report of the New Jersey station shows points of resemblance to, and of difference from, that of the Connecticut station. As in the former case, the largest draught upon the resources of the station has been for the analysis of fertilizers, a hundred and ninety-four of which have been examined. Unlike the Connecticut station, the New Jersey station had ready to its hand tolerably good facilities for conducting field and feeding experiments;

and to these, and the auxiliary chemical work which they involve, the residue of its energies has been directed. These experiments include comparisons of green rye with rye ensilage, and of dried fodder-corn with corn ensilage, as food for milch-cows; field trials with fertilizers on various crops and on various typical soils in the state; experiments upon sorghum as a sugar-producing plant, and preliminary work on sweet-potato disease.

Not all of these experiments are of the highest order; but they are accurate and painstaking, and they touch the actual interests of the farm more closely than any mere laboratory work, however excellent, can do. The experiments on sorghum were mainly upon the effect of fertilizers upon the yield of sugar, and gave the interesting result that the yield of sugar was more favorably affected by potash than by any other single substance, and that, with the addition of nitrogen to the potash, the largest yield of sugar per acre was obtained. Sulphate of potash surpassed the 'muriate' in every case. Both sorghum bagasse and seed (the whole plant cut for fodder) and sorghum ensilage proved very satisfactory fodders for cows and pigs.

INTELLIGENCE FROM AMERICAN SCIENTIFIC STATIONS.

GOVERNMENT ORGANIZATIONS.

Geological survey.

Mineral springs in eastern Tennessee. — Mr. F. M. Pearson, who carried on topographic work for the survey last summer in eastern Tennessee, reports that the section of the state upon the map of which he is now engaged is full of mineral springs belonging to the classes of sulphur and chalybeate springs. He mentions particularly Bean's Station Valley, in Grainger and Hawkins counties, as being the locality of some twenty springs, a number of which have been improved, and are places of resort. On the north-western side of the valley lies the 'Poor Valley Ridge,' which extends for a distance of some thirty miles from the north-east to the south-west. This ridge is separated from the Clinch Mountain, which is on the north-west and parallel with it, by a depression or hollow known as 'Poor Valley.' In the latter, numerous small streams rise, separated by low divides, which, after flowing in the valley for short distances either south-west or north-east, turn and reach Bean's Station Valley through gaps in the Poor Valley Ridge. At every one of these gaps on the south-east side of the ridge, sulphur springs are found. Most of these springs are unimproved, as far as conveniences for using the waters are concerned; but those at which hotels have been built are among the most popular places of resort in the state. Beginning at the south-west, where the ridge abuts against the Clinch Mountain, the first springs of importance are 'Lee's Springs,' which are situated at the extremity of the Poor Valley Ridge, or rather partly between it and the Clinch Mountain. Powder Spring (named from the odor of the sulphuretted-hydrogen gas), at Powder Spring Gap, five miles farther to the north-east, is the next important locality. Following the ridge fifteen miles from this point, toward the north-east, brings one to 'Tates Springs,' one of the most noted localities in Tennessee. There are good accommodations here; and stages connect with the Eastern Tennessee, Virginia, and Georgia railroad at Morristown. Stages connect 'Lee's Springs' with Strawberry Plains, a station on the same railroad.

Hale's red and white sulphur springs, in Hawkins county, five miles north of Rogersville, are also resorted to, and are on the same line as the other springs enumerated. There are also several chalybeate springs on or near the same line, in Hawkins and Grainger counties. Other well-known watering-places, determined by the presence of mineral springs, in the region surveyed by Mr. Pearson, are 'Montvale Springs' in Blount county, 'Oliver Springs' in Anderson county, 'Austin Springs' in Washington county, and 'Galbraith's Springs' in Hawkins county.

Burk's Garden, in Virginia. — The peculiar topographical system of long narrow valleys, with streams flowing from their opposite ends to the middle, and thence at right angles across or through one of its boundary ridges, which is one of the striking features of the physical geography of the country surveyed by Mr. Morris Bien in the southern Appalachians (described in *Science*, No. 56), gradually changes, as it is traced north-eastward from the valley of east Tennessee, until in Tazewell county, the northern county of south-western Virginia, is found the most southerly instance of a topographical feature common in Pennsylvania. This is Burk's Garden, a beautiful oval valley, eight miles long by four and a half miles wide. It is surrounded by a ridge averaging more than twelve hundred feet in height. The valley contains some of the richest blue-grass land in the state. Its drainage forms one of the heads of Wolf Creek, which affords numerous examples of sink-hole drainage, so common in the area surveyed by Mr. Bien. This stream leaves the valley by flowing through the western side of the oval range in a deep and rugged gap, or cañon as it would be called in the west. This valley well deserves the name of 'garden,' for it is one of the most beautiful spots in Virginia. The first glance recalls Johnson's description of 'Happy Valley' in Rasselais, and it is without doubt destined to become a popular mountain resort.

It is located in the southern part of Tazewell county. South-east of it is the valley of Holston River, in which there are large gypsum deposits. The fertility of Burk's Garden may perhaps be due to the presence of gypsum. Elk Garden, in Russell county, south-west of Tazewell, is somewhat like Burk's Garden, but not so well defined, although it may have been so in the past.

RECENT PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Trenton natural history society.

April 8. — Mr. F. A. Lucas arraigned the English sparrow, Passer domesticus, as a nuisance; stating, that, after several seasons of careful scrutiny, he had never seen the bird capture or destroy a single larva. It will chase butterflies, fight with our native birds, and drive them away; it will devour the grain of the farmer, and the seeds cultivated for commercial purposes; but to do any thing useful is against its principles. It is stated by J. H. Gregory, a veteran seed-grower, to be one of his greatest enemies. Mr. Lucas referred to contests which he had witnessed between Passer domesticus and Picus pubescens, the hard-working Certhia familiaris, Troglodytes aedon, and Regulus satrapa; in some instances several sparrows uniting in the attack. He had also seen the bird perched on a tree whose branches were loaded with webs of certain caterpillars, without even noticing them, but waiting for the crumbs from the breakfast-table. The pestiferous foreigner is, in this state, protected by law, a penalty of five dollars being imposed for the killing of one. —— Dr. A. C. Stokes communicated a paper on Cynips quercus-cornigera of Osten-Sacken, exhibiting the spinous galls and several microscopic dissections of the fly, especially of the ovarian tubules, to show the arrangement of the pedunculated eggs. The numerous tubules are in two clusters, radiating from common centres. The peduncle of each egg is twisted about the egg next to the rear; so that, when deposited on the twig, the stem is directed upward, and develops into the hollow, thorn-like body projecting from the gall. The fly escapes by gnawing a small hole in one side of this body, above the surface of the excrescence. Several mature flies were removed from these woody capsules in December; and from the same gall, at the same time, were taken larvae and pupae. —— At a previous meeting, Prof. Austin C. Apgar referred to the spawning of Fulgur caniliculata, stating that the eggs were deposited every month, except, perhaps, in the winter. A recent experience on the New-Jersey coast has led to the belief that spawning may take place at any season. In the region about Cape May these long clusters of egg-cases are popularly supposed to be the skeletons of defunct snakes.

Engineers' club, Philadelphia.

April 5. — Mr. Henry G. Morris gave a brief description of an atmospheric elevator, consisting of a closed cage or car working in an air-tight well; the air-pressure, supplied by a 'Root' or other pressure blower, being admitted to the top or bottom of the cage in descending or ascending. The doors at the different stories opening inwards, the pressure of air keeps them closed until the interior of the car is brought opposite, when, the pressure being relieved, the door can be opened into the car. The car being counterbalanced, only a comparatively slight pressure of air, equal to a water-column of from six to eight inches only, is required to move an average load on a car six feet square. The escape of air beneath the car being at all times readily controlled by the attendant, it is impossible for the car to descend at a dangerous speed; and other obvious features render this form of elevator comparatively safe. —— Mr. Henry G. Morris also exhibited a sample of seamless copper tube which had been compressed endwise under a steam-hammer, and showed peculiar foldings of the metal into overlapping equilateral triangles forming an interior hexagonal section. —— Mr. John T. Boyd described a new design for parlor-cars for the Pennsylvania railroad. —— The secretary presented for Mr. Edward Parrish an illustrated description of Powers's disinfecting-tank and automatic siphon. —— Mr. William L. Simpson exhibited a remarkably perfect casting of a toad, the pattern used being the toad himself.

Minnesota academy of natural sciences, Minneapolis.

March 4. — Mr. C. L. Herrick mentioned the recognition of a genus of lynceid crustaceans, Monospilus, new to America. M. dispar is peculiar among Cladocera, in that, living in the filth at the bottom of pools, it not only fails to completely moult its periodically produced coverings, but fails to develop the compound or imago eye, while the macula nigra persists through life as the functional visual organ. This most interesting form has outward resemblances to Iliocryptus, while its real affinities seem to be with the higher lynceids from which its habits have degraded it. Mr. Herrick regards the Minnesota form as identical with that of Europe. He also presented a tabular statement of the distribution of the fresh-water crustacea of the orders Cladocera and Copepoda; showing a remarkable conformity between the faunas of Minnesota and Scandinavia, and a very large percentage of identical species. Southward, toward the Gulf of Mexico, the number of species becomes less, while the percentage of new species increases. Several species rarely found in Minnesota become common southward, and these are always species differing from those of Europe. Such species, however, represent usually intermediate species between extremes found associated at the north, or links between genera. Such species are Simocephalus daphnoideus Herr., which is a link between Simocephalus and Daphnia and Scapholeberis angulata Herr., which stands related to

Simocephalus. Pseudo-sida bidentata Herr. unites Sida and Daphnella. Mr. Herrick inclines to the opinion that the fauna of the states south of the Ohio River is a remnant of a pre-glacial one; while in the drift-covered areas a new circumpolar fauna has arisen, measurably independent of the previous one, though, of course, derived from it. The paper led to some discussion of geological evidence of the origin and persistence of types of fresh-water animals, and a comparison of the specialized phyllopod fauna of America with the cosmopolitan character of other fresh-water groups. —— Mr. Warren Upham spoke of the progress made in cataloguing the plants of Minnesota, a work on which he is engaged. Much interest is shown by the botanists in all parts of the state in contributing material and notes. The total number of species of flowering plants and ferns now known to occur in Minnesota, growing without cultivation, is 1,527, belonging to 546 genera, which represent 115 families or orders. Of these, 125 species are introduced, being foreign plants that have become established or naturalized, leaving 1,402 that are aborigines. Up to the present time, only about half as many introduced weeds are known in Minnesota as in New England; the difference being due to the later settlement of the former section. —— Mr. John B. Leiberg contributed a paper on plant-life in Montana and Dakota. It was stated that many species found were met with in the south-west only at high elevations. Their growth was of a luxuriance not seen in Minnesota. Only one kind of cherry was found west of the Missouri River along the line of the Northern Pacific, this being the little sandy cherry. Golden-rod was abundant. But one kind of pennyroyal was met. Fully one-half the grass found west of the Missouri was of one kind. Only two species of ferns, and but few mosses, were seen. The great number of fossil trees between Bismarck and Llendive was a fact of particular interest. From the stumps, some of them ten feet in diameter, the trees originally must have been of immense size.

NOTES AND NEWS.

IN addition to the signal-service note mentioned in our editorial columns, there is another, no. vi., by the same author, on wind-velocities as determined during the summer of 1882, by hourly records of automatic anemometers at Chicago, and on the lake crib, three miles out on Lake Michigan, whence the city's water-supply is taken to the shore by a tunnel. The discussion shows the local peculiarities of the wind with much distinctness. The general ratio of velocity in Chicago to that at the crib is about 1 : 2, even though the anemometer in the city is a hundred and three feet above the ground, while that on the lake is only fifty-seven feet above the water; proving a marked control exercised by even so smooth a land-surface as that about Chicago in retarding the winds, — a control probably much strengthened by the buildings in the city. The diurnal variation of velocity is shown clearly at both stations: the maximum occurring about three or four in the afternoon, on land, and about four or five over the water; the minimum being rather uniformly maintained from ten in the evening, on through the night. The ratio of increase is much greater at the former (5.6 : 9.6) than at the latter station (11.5 : 13.5), as might be expected, both from the greater diurnal changes of temperature on land, and from the fact that at the time of maximum velocity on land the lake-breeze prevails. Directions are given only for the city station: they exhibit the phenomena of land and sea breezes in good form. The average of four months, here copied in fig. 1 with slight change, shows the south-west land-breeze from four in the morning till eleven; then there is an abrupt reversal to the north-east lake-breeze, which persists from noon till ten at night, followed by a gradual right-handed veering as the land-breeze is established again. The veering is found with greatest regularity in the July averages. Fig. 2 illustrates the immediate reversal from west-north-west to east-south-east at noon, followed by the

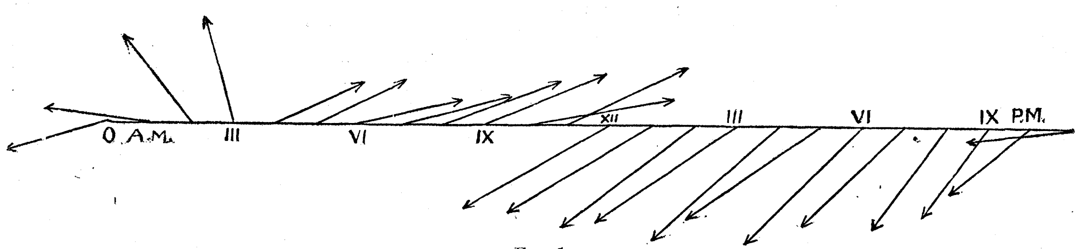


FIG. 1.

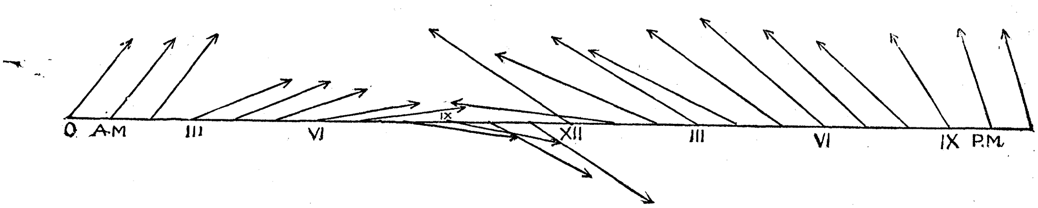


FIG. 2.

gradual hauling-around to west-north-west again in the succeeding twenty-four hours. The arrows are here drawn proportional to the velocities (maximum, 9.9 miles an hour; minimum, 5.6), as they should better have been in the original. The veering in the other months is much less regular. The little pamphlet affords excellent material for use in teaching, as well as for use in improving weather-predictions.

No. xi., by Lieut. F. K. Ward, of the same series of notes, treats of the elements of the heliograph, for use in military signalling, with the advantage of silently calling the attention of those to whom the signals are addressed without being visible to the enemy. No. xii., the latest of the series, by Sergeant J. K. Finley, is on the special characteristics of tornadoes, giving a concise description of their peculiarities. We should have been glad to see in it a statement of what the signal-service is attempting, in the way of tornado studies, by means of its special reporters.

—R. Baron, writing to *Nature* from Antananarivo, Madagascar, of a curious habit of insects, says, "One morning, while sitting by the side of one of these streams, I noticed a papilio, which is an insect measuring about four inches from tip to tip of its wings, resting on a wet bank; and, wishing to procure it as a specimen, I approached it as gently as possible, the creature being apparently so absorbed in what it was about as to be totally unconscious of my proximity to it. Noticing strange and unaccountable movements, — sundry jerks and probings with its proboscis, — I quietly sat down near it to watch it more closely. I observed that every second or two a drop of pure liquid was squirted (not exuded merely) from the tip of its abdomen. I picked up a leaf that was lying near, and inserted the edge of it between the insect's body and the ground, so as to catch the liquid. Unfortunately, I had no watch with me at the time, nor means of measuring liquids; but I reckoned that about thirty drops were emitted per minute. I held the leaf for about five minutes, — as nearly so as I could reckon, — and at the end of that time there was caught in it about a saltspoonful of what seemed to be pure water, without either taste or color. After watching the butterfly for a time, I seized it by the wings between my thumb and fingers with the greatest ease, so utterly lost did it appear to be to what was going on near it. In another spot I saw as many as sixteen of these large butterflies within the space of a square foot, all engaged in the same strange action."

—According to the London *Academy*, an ancient human skull has been found at Podhaba, near Prague. It was unearthed in a bed of chalk where the tusk of a mammoth had been dug out a few days previously, which gives an indication of its age. The characteristics of this skull are the extremely low forehead and the excessive development of the ridges, in both of which points it resembles the famous Neanderthal skull, though its facial angle is yet lower.

—The Entomological society of Washington has organized with the following officers: president, Dr. C. V. Riley; first vice-president, Dr. J. G. Morris; second vice-president, George Marx; recording secretary, E. A. Schwarz; corresponding secretary, L. O. Howard; treasurer, Benjamin P. Mann; executive committee, the officers and Dr. W. S. Barnard, P. R. Uhler, and Dr. A. J. Shafhirt. The first regular monthly meeting of the society was held April 3, in the council-chamber of the U. S. national museum.

The active membership list of the society numbers over twenty names. Regular meetings are held on the first Thursday evening of each month.

—The pilot chart of the hydrographic office for April embodies several neat improvements on the preceding numbers. The printed supplement is replaced by a greater detail of conventional figures, with dates, printed in red on the chart; so that there is no longer necessity of looking elsewhere for needed information. The rig and attitude of wrecks are graphically represented, the name and date of observation being placed beside them. Wrecks observed more than once are plotted in all their positions with dates, and connected by a dotted line. Thus, from Jan. 7 to March 12, the schooner Maggie M. Rivers had drifted from off Cape Hatteras about five hundred miles east-south-east, obliquely *across* the ordinary course of the Gulf Stream. An intermediate position was noted on Feb. 6. One water-spout is recorded for March 3, two hundred miles east of Norfolk. It would be worth while to give the hour of such transient phenomena. Bergs and field-ice were very plentiful south-east of Newfoundland. Hereafter the charts will be sent to press the first of every month. The news of the previous month will be given as far as received, and any thing coming in later will appear on the next issue.

—It is rather late, but perhaps not quite too late, to call attention to the exceedingly important article by S. P. Langley, on the determination of wave-lengths in the invisible prismatic spectrum, in the March number of the *American journal of science*, simultaneously published, also, in the *Philosophical magazine* and some of the continental journals.

It gives the first, and so far the only, reasonably accurate wave-length determinations in the lower invisible portion of the spectrum. The results were obtained by a very ingenious and unexceptionable combination of grating and prism, and their correctness is beyond dispute within the limits of accuracy assigned. They show conclusively that the corresponding wave-lengths published by previous (and some contemporaneous) investigators are, at best, only roughly approximate, because founded on extrapolation from formulae which break down in the region of longer wave-lengths. The formulae of Redtenbacher, Cauchy, and Briot, were all investigated, and all fail; Briot's turning out the least inaccurate. Professor Langley's work makes it evident that the theory of dispersion needs revision and perhaps reconstruction.

Some of the results given in this article have been published before, within a year or two, in a fragmentary way, in the *Comptes rendus*, and in papers read before the National academy and elsewhere; but we

have now, for the first time, a connected statement of the whole investigation, which lays a foundation for future extended work in the same direction.

A casual reader would hardly be likely to appreciate the immense amount of labor involved in the research, both in observation and computation: but all acquainted with this sort of work will know that it must have been exceedingly laborious, tedious, and delicate; and specialists will await with great interest the publication of the unabridged memoir, in the Transactions of the National academy of sciences, with all the original records and details of the observations.

— In a paper published in *Van Nostrand's magazine*, Professor Thurston introduces a report, by Messrs. Brooks and Steward, on tests of an Otto gas-engine made at the Stevens institute of technology in the spring of 1883. The machine was furnished by the builders, and was subjected to a careful test, determining the method of distribution of heat, in useful effect and in wastes. Earlier determinations, under the direction of Professor Thurston, had been made, with results, in one case, given in illustration, as follows: —

Useful (dynamometric) work	14.27
Work of pump	0.42
Friction of engine	4.10
Heat 'exhausted' from engine	23.55
Heat wasted by water-jacket	46.90
Loss by radiation, etc.	10.76
Total heat supplied	100.00

The consumption of fuel varied from twenty-one to twenty-four and a half cubic feet per horse-power and per hour. The friction of mechanism was four or five per cent of the total energy of the fuel, or about thirty per cent of the useful power. The water-jacket carried off from forty-five to fifty-five per cent of the heat of combustion. The engine delivered seven to nine horse-power.

The trials of 1883, at the Stevens institute of technology, were made with an engine rated at ten-horse power. The air and gas were *both* measured by meter, — probably the first time that this had been attempted. It was found that the real proportions of air and gas were not determinable, except by metering both, as here done. The fact was proven that combustion continues, even after expansion has progressed to a very considerable extent, — a fact that had been before suspected, but probably never before proven. The distribution of heat was as follows: —

'Indicated' work	17.00
In exhaust	15.50
In water-jacket	52.00
Lost by radiation, etc.	15.50
Total heat	100.00

In the 'indicated' work are included useful work, and friction of engine, the latter amounting to about 0.20 of the former.

The cost of operation of the gas-engine is given at 8.75 cents per horse-power and per hour, — considerably more than the steam or the hot-air engine, when working continuously; but the comparison is more favorable to the gas-engine for discontinuous work. The expense of the gas-engine will also be greatly reduced by the introduction of special 'heating-gas,' which can be supplied at one-half the cost of illuminating-gas.

The report affords an unusually full collection of valuable data for use in the construction of the theory of the gas-engine. It is remarkably well worked up, giving the equations of the expansion-lines; composition and specific heats of the gases; pressures, volumes, and temperatures at the various portions of the cycle; and all items of cost.

— At the meeting of the Linnaean society of London, on March 6, Professor Cobbold gave a verbal account of a communication from Dr. P. Manson of Hong Kong, in which the author furnishes fresh evidence as to the *rôle* of the mosquito considered as the intermediary host of Filaria sanguinis-hominis. Dr. Manson has verified his previous observations in the most complete manner, and he now recognizes and describes six well-marked stages of the Filariae whilst they are dwelling within the body of the insect. In the discussion following, Dr. T. R. Lewis confirmed Manson's statements in many particulars.

— M. Tisserand, assisted by MM. Bijourdan, Callandreau, and Radau, issued on Feb. 15 the first number of a new astronomical monthly, entitled 'Bulletin astronomique,' to be published under the auspices of the Paris observatory.

— *Nature* announces that at the final meeting, March 21, of the general committee of the International fisheries exhibition, the balance of the funds was disposed of. The surplus amounts to over £15,000; and of this, £10,000 were allotted to alleviate the distress of widows and orphans of sea-fishermen, while £3,000 were voted as an endowment to a society which is to be called 'The royal fisheries society,' whose functions will be somewhat similar to those of the Royal agricultural society. The remaining £2,000 are kept in reserve.

— For the purpose of a scientific inquiry into the amount and fluctuation of the rainfall in different parts of the world, A. R. Binnie, Town Hall, Bradford, Yorkshire, Eng., wishes to collect long and continuous records of rainfall extending from as early a date as possible. 1°. The records should state the annual falls only, as taken year by year without a break, for periods of at least fifteen years; but the longest possible period is most desired. 2°. The name of the place of observation, with, if possible, the latitude and longitude, and its elevation above the sea-level, should be given. 3°. The total annual fall should be expressed in millimetres, English inches, or local or obsolete measures; but if in either of the latter, their equivalent in millimetres or English inches should be given. 4°. The name of the observer, or authority, or publication from which the record is obtained, should be given. 5°. The records should be from observations made at a single station, and should not be compiled from the records of two stations; but the greatest number of different records taken at different stations is desirable, to avoid local errors or peculiarities.

— The gunpowder-mills owned by Messrs. W. H. Wakefield & Co., near Kendal, Eng., are now lighted by the electric light; they being the first works of the kind where this mode of illumination has been adopted. The works are very extensive, at least two miles in length. The dynamo is placed about the centre of the works. Very long mains were necessary, as each dangerous building is about two hundred yards from its neighbor. Over head, bare wires were found to be the best for conveying the current. These were carried on insulators on posts and trees along the route, four to eight lamps being necessary to each. The lamps used are the new pattern, twelve-candle power Swan lamps. The dynamo runs almost continuously day and night in the winter, the average work per day being at least twenty hours. In the dangerous powder-making sheds the lights are enclosed in specially designed copper reflectors, enamelled white inside, with tight-fitting plate-glass fronts. Each lamp is under separate control, and each circuit can be controlled by a switch in the machine-room. Every lamp and every circuit is also protected by a safety-plug, which melts in case of danger through excess of current; thus breaking the current, and removing all possible danger.

— The rainfall in San Diego, Cal., and also throughout southern California, is greater for the present season of 1883-84 than has ever been previously recorded. A total of 18.46 inches has fallen at San Diego, and as high as 60 inches have been reported from the back country. The rainfall for 1879-80 was 14.89 inches; 1880-81, 9.30 inches; 1881-82, 9.47 inches; and for 1882-83, only 4.91 inches.

— The Indians in Oregon are much disturbed by the constant settling of whites on lands which they have occupied, and which have enabled them to gain a living by horse-raising. They recently asked for a hearing for their grievances from the commander of the fort at Walla Walla, which was granted. They were told, however, that their only remedy was in taking the land as individuals, and not as members of a tribe. But as they have scruples about dealing in mother-earth, from which they all come, and to which they return, the prospect is at present that they will be finally driven from all land outside their reservation.

— Professor Ormond Stone, now of the University of Virginia, resigned the position of astronomer of the Cincinnati observatory in June, 1882; and upon his advice, his former assistant, Mr. Wilson, now astronomer *pro tempore*, has devoted himself chiefly, since that time, to the reduction of the miscellaneous observations which remained unpublished. No. 7 of the publications of the observatory, a pamphlet of 79 pages, contains those observations which pertain to comets, and is divided about equally between observations of cometary positions and physical observations. Previously to 1880 this observatory paid no attention to these bodies, the equatorial (Merz and Mahler, $11\frac{1}{4}$ inches aperture) being principally engaged with double-star observations. The former publications of this observatory (Nos. 1-6) relate entirely to discoveries and micrometrical measurements of double stars.

The observations of position were made after the usual manner, mostly with the filar, but sometimes with the ring-micrometer, and need no further mention here. The assumed co-ordinates of a hundred and fifty-four comparison-stars are given also. The physical observations, generally made just before or after the observations of position, consisted of sketches, measures, and notes on the appearance of the comets. Sketches of the heads of comets were made with the large equatorial, using a power of about a hundred diameters. The tail-sketches were made with the unassisted eye, and sometimes an opera-glass. All the stars visible in the vicinity of the comet were plotted upon the pencil-sketches as accurately as possible with the eye. The stars were afterward identified in Heis's *Atlas Coelestis*, and plotted to a scale three times that of the engravings. The position of the nucleus was then plotted, and the tail drawn in the same proportion, relatively to the stars, as on the original sketch. In the process of photo-engraving, the compiled sketches were reduced to one-third, so that the engravings are about the same size as the original sketches.

The theory and methods of discussion of tail-observations of comets, elaborated by Dr. Bredichin, director of the observatory of Moscow, have been followed by Mr. Wilson; and he summarizes that theory from *Copernicus* and the *Annales de l'observatoire de Moscou*.

The discussions of the notes on the several comets form a very interesting contribution to cometary astronomy. The plates accompanying the work contain about thirty drawings of comet (*b*) 1881, twelve of comet (*a*) 1882, and twenty of comet (*c*) 1882, commonly known as the great comet of that year; and they appear to have been reproduced in a manner worthy of the accuracy of the originals.

— In the French journal, *La ramie*, M. Pailleux calls attention to a Japanese plant named Kusu (Pueraria Thunbergeana), the roots of which contain starch, while the leaves and shoots are used as food. Its fibrous portions are adapted for use in the manufacture of cordage. It is a lofty and hardy plant, attaining within a year to the height of between twelve and twenty-five feet. It yields fruit, and grows upon the most unfruitful dry ground, where nothing else would thrive, provided there is a sufficiency of warmth. It requires no care, and can be propagated by seeds or by planting.

— The Chinese are beginning to adopt western chemical science, and a factory has recently been erected for the manufacture of sulphuric acid on a large scale. Two well-known chemical text-books — Malguttis' Elementary chemistry, and the Chemical analysis of Fresenius — have also been translated into Chinese, with the help of a great number of new characters, and adopted in the imperial colleges. His excellency Tong Kin Sing, the first minister, has taken the work under his immediate patronage, and written the preface for the first of these books.